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Enjoyment of Work by Full-Time Workers in the U.S., 1955 and 1980

NORVAL D. GLENN AND CHARLES N. WEAVER

A MAJOR thesis of a best-selling "pop sociology" book published in 1956 (Whyte, 1956) was that the Protestant work ethic was declining in Amercia, and that thesis has been reiterated by scores of journalists and social scientists since then. In recent years, the assumption that there has been a decline in interest in and commitment to work has become so prevalent that discussions of it in the social scientific literature are sometimes worded as though the decline is common knowledge and requires no documentation by citations of evidence.

However, much of the relevant "hard" evidence fails to support this prevalent view. For instance, there has been no systematic change in responses to a global (general) job satisfaction question which has been asked, with slight variations in wording, on several

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Abstract Two questions concerning enjoyment of work asked on a 1955 American Gallup Poll were asked on a 1980 United States national survey to gauge the net change during a quarter of a century. Only the responses of persons employed full-time were used for this study. Indicated enjoyment of work was substantially lower in 1980 than in 1955. The difference was especially great for manual workers, Protestants, and older persons. A tentative conclusion drawn from examining trend data for one birth cohort is that the change resulted from cohort succession and from orientations and attitudes members of younger cohorts brought to their work rather than from changes in work conditions which affected workers of all ages and in all birth cohorts. Some implications of the findings are disscussed.

Norval D. Glenn is Professor of Sociology, University of Texas, Austin. Charles N. Weaver is Professor of Management, St. Mary's University, San Antonio. The research reported here was supported in part by National Science Foundation Grant SES-7907917. The 1955 data on enjoyment of work were obtained from the Roper Center at the University of Connecticut. The authors are indebted to the Gallup Organization, Inc., and especially to James Shriver, III, for cooperation in gathering the 1980 data on enjoyment of work. Data from the 1978 and 1980 General Social Surveys, conducted by the National Opinion Research Center with funds from the National Science Foundation, were also used in the research.

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American national surveys since 1958.[1] Quinn et al. (1974) reanalyzed job satisfaction data gathered by 15 national surveys from 1958 to 1973 and concluded that there was no convincing evidence for a decline in job satisfaction during that period. Katzell (1979) extended the series by adding data from surveys conducted in 1974, 1975, 1976, and 1977, and he still found no evidence for any substantial change. Data from the General Social Surveys conducted in 1977, 1978, and 1980 (Davis, 1980) show an aggregate level of reported job satisfaction at about the same high level at which it has existed for over 20 years.[2]

On the other hand, survey data other than responses to the global job satisfaction question do indicate a recent decline in satisfaction with work. Quinn and Staines (1979), using a composite index based on responses to several questions about specific aspects of job satisfaction asked on the Quality of Employment Surveys conducted by the Survey Research Center at the University of Michigan, found a distinct decline in the mean index scores from 1973 to 1977, and commercial opinion polls also found some changes in attitudes toward work during the 1970s.[3] As a whole, the relevant survey data are rather inconsistent and may seem confusing (even though the different data can be reconciled with one another). Data on behavioral indicators of work orientation, such as rates of absenteeism and job turnover, do not make the picture much clearer, because data can be found either to support or to refute the alleged decline in commitment to work (Strauss, 1974).

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[1] The most common wording of the question has been: "On the whole, how satisfied are you with the work you do—would you say you are very satisfied, moderately satisfied, a little dissatisfied, or very dissatisfied?" However, the Gallup version of the question only asked the respondents whether they were satisfied or dissatisfied, and thus the responses to the different variants of the question can be made comparable only by collapsing all responses into "satisfied" and "dissatisfied."

[2] Data from the 1974, 1975, and 1976 General Social Surveys are included in Katzell's series (see his Table 1). The percentage of GSS respondents who said they were satisfied was 87 in 1977, 87 in 1978, and 83 in 1980. The apparent decline from 1978 to 1980 was probably real ($p = .060$—two-tailed), but the 1980 level was no lower than that in some other years since 1958, including the 81 percent in 1958, and it was only slightly lower than the 85 percent in 1974.

Data on the four cohorts studied by the National Longitudinal Surveys show a decline in job satisfaction in each cohort from 1966 to 1972 (Andrisani, 1978), but since data from repeated cross-sectional surveys show little change during that period, the meaning of the changes shown by the panel data is unclear. The changes could be age effects, but the usual inference made from cross-sectional data is that aging, or some close correlate such as increasing extrinsic job rewards, typically leads to an increase in job satisfaction (Glenn et al., 1977; Wright and Hamilton, 1978). One might even speculate that the repeated interviewing of the panel members on topics relating to jobs and work sensitized them to deficiencies in their work situations and led to a decline in their mean job satisfaction.

[3] For a summary of some of the poll data, see Mindell and Gorden (1981).

If there have been important recent changes in American workers' orientations to their jobs and work, responses to the global job satisfaction question obviously do not reflect them. Perhaps, as several authors have suggested, the stability in the global job satisfaction data reflects offsetting trends in satisfaction with different aspects of work. For instance, to use Herzberg's well-known distinction (Herzberg, 1966), satisfaction with extrinsic rewards of work (pay, prestige, etc.) may have increased while satisfaction with intrinsic rewards (direct enjoyment of work) has declined.

Although data from the Quality of Employment Surveys—conducted in 1969–1970, 1973, and 1977—enable one to distinguish between satisfaction with extrinsic and intrinsic rewards of work (and to distinguish among other kinds of satisfaction as well), comparable data from national samples are not available for dates earlier than 1969–1970. Therefore, there have been no available American national data with which to trace the level of such variables as enjoyment of work during the past quarter of a century or for any period except 1969–1977.

When needed data on trends are not available, they can often be created. First, one finds appropriate data from an earlier period and, by having the question or questions repeated on a contemporary survey, one generates comparable data. That is what we did for this study. In June 1955, the American Institute of Public Opinion (the American Gallup Poll) asked a national sample of about 1,500 respondents two questions about enjoyment of work. We commissioned inclusion of these two questions, in the same order and in about the same position on the interview schedule, on an American Gallup survey conducted in May 1980—almost exactly a quarter of a century after the first asking of the questions.[4]

The questions are:

> Do you enjoy your work so much that you have a hard time putting it aside? Yes. No. Don't know.
>
> Generally speaking, which do you enjoy MORE—the hours you are on your job, or the hours when you are NOT on your job? On job. Not on job. Don't know.

---

[4] The 1955 data are from AIPO Survey 548, conducted June 3–8, 1955, and are available from the Roper Center, University of Connecticut, Storrs, Connecticut 06268. The 1980 data were collected on Gallup Personal Omnibus Survey 155-G, conducted May 16–19, 1980, and will eventually be available from the Roper Center. Both surveys used face-to-face interviews and modified probability sampling. A minor difference between the two surveys is that the 1955 sample included persons age 21 and older while the 1980 sample included persons age 18 and older. Of course, the respondents ages 18–20 can be excluded from the 1980 sample, but they are not excluded from the data reported here since their inclusion makes a negligible difference.

These questions are perhaps not ideal for measuring enjoyment of work, but they have the advantage of being simple and straightforward, and although the responses to them were distinctly different in 1955 and in 1980, at neither date were the responses highly skewed, as responses to the global job satisfaction question always are. Many people may be reluctant to say they are dissatisfied with their work, but they should be less reluctant to say they do not have a hard time putting their work aside and enjoy hours off the job more than hours on the job. We cannot be certain that there is no substantial "social desirability bias" (Phillips, 1973) in the responses, but any such bias should be less pronounced than the comparable bias in responses to the global job satisfaction question. And any changes in responses from 1955 to 1980 due to changes in social desirability bias must reflect important changes in work values just as surely as real changes in enjoyment of work must reflect attitudinal and value changes.

The 1955 and 1980 data give only two snapshots of stated enjoyment of work by American workers, and thus there is some danger in using them to infer long-term change. If enjoyment of work were subject to pronounced short-term fluctuation in the working population as a whole, any differences between the two sets of data might simply reflect one survey being conducted at a low point in the fluctuation and the other being conducted at a high point. We can think of no reason, however, why reported enjoyment of work should be subject to much short-term fluctuation. Perhaps the most likely kind of fluctuation is seasonal, and the 1955 and 1980 surveys were conducted at almost the same time of year (less than a month apart on the calendar).

Fortunately, the 1955 survey was conducted after Gallup began to use modified probability samples and adequately to represent all major demographic categories in their samples (see Glenn, 1974; 1977). Although there were some fairly minor changes from 1955 to 1980 in the way Gallup selected respondents in the final sampling stage, there are no major problems of comparability between the 1955 and 1980 samples. However, since both are multiple-stage cluster samples, significance tests computed with formulas designed for simple random samples will not yield accurate probabilities with these data. To estimate the significance of differences between proportions in the two samples, we used the formula from Blalock (1979: 232–34) and multiplied the standard errors computed by that formula by 1.5 as a correction for the "design effect" (see Kish, 1965; Frankel, 1971). In most cases, the correction should be adequate, but a few of the reported probabilities may be at least slightly lower than the correct ones.

Findings

The 1980 data show substantially lower reported enjoyment of work than do the 1955 data. The percentage of full-time workers who said they enjoyed their work so much they had a hard time putting it aside declined from 51.7 to 33.5, a difference of 18.2 points ($p < .001$—two-tailed). That is, the proportion declined by more than a third. The percentage of full-time workers who said they enjoyed hours on the job more than hours not on the job declined from 39.8 to 24.1, a difference of 15.7 points ($p < .001$—two-tailed). Again, the proportion declined by more than a third.

One might suspect that compositional changes in the work force contributed substantially to these changes, since the percentage of full-time workers who were women and who were young adults increased during the quarter of a century covered by these data.[5] However, responses to the work enjoyment questions did not differ much by sex in either 1955 or 1980 (Table 1), and the change in the age composition of the workforce contributed only modestly to the decline in reported enjoyment of work. When the 1955 and 1980 responses are standardized to the same age distribution (the mean of the distributions at the two dates), the 1955–1980 change in percentage of persons who said they enjoyed their work so much they had a hard time putting it aside is still 17.1 points, and the change in the percentage of persons who said they enjoyed hours on the job more than hours not on the job is still 14.9 percentage points.

Changes in the percentages of responses indicating high enjoyment of work within categories delineated on the basis of age, occupation, and religious preference are shown in Tables 2 and 3. All the indicated changes are declines, although not all are estimated to be statistically significant, and the change indicated for farmers in Table 3 is negligible.

Consistent with a widespread belief that the "retreat from work" has been greater among blue-collar workers than among others (Strauss, 1974), the greatest indicated change among the occupational categories in both Tables 2 and 3 is for manual workers. All the differences in change between manual workers and other categories fall short of statistical significance according to our estimates, however, and thus we cannot be confident that they existed in the population. In any event, at both dates the level of reported enjoyment of work was lower for manual workers than for professional and busi-

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[5] The percentage of women is 23.4 in the 1955 sample and 34.4 in the 1980 sample. The percentage of persons under age 30 is 17.4 in the 1955 sample and 30.1 in the 1980 sample.

Table 1. Responses of Full-Time Workers to Questions about Enjoyment of Work, by Sex, U.S. National Samples, 1955 and 1980

	Males			*Females*		
	1955	*1980*	*Change*	*1955*	*1980*	*Change*
Do you enjoy your work so much that you have a hard time putting it aside?						
Yes	52.6%	34.1%	-18.5^{a}	48.9%	32.4%	-16.5^{b}
No	44.3	63.8	19.5	49.5	64.9	15.4
Don't know	3.2	2.1	−1.1	1.6	2.7	1.1
Total	100.0	100.0		100.0	100.0	
(*N*)	(603)	(848)*		(184)	(445)*	
Generally speaking, which do you enjoy MORE—the hours you are on the job, or the hours when you are NOT on your job?						
On job	38.0	22.9	-15.1^{c}	45.7	26.5	-19.2^{d}
Not on job	50.4	65.6	15.2	45.7	62.5	16.8
Don't know	11.6	11.5	−0.1	8.6	11.0	2.3
Total	100.0	100.0		100.0	100.0	
(*N*)	(603)	(848)*		(184)	(445)*	

[a] $p < .001$ (two-tailed).
[b] $p < .02$ (two-tailed).
[c] $p < .001$ (two-tailed).
[d] $p < .01$ (two-tailed).
* Weighted sample. The *N* for the unweighted sample was used for the significance test.

Table 2. Percentage of Full-Time Workers Who Said They Enjoyed Their Work So Much They Had a Hard Time Putting It Aside, by Selected Demographic and Social Characteristics, U.S. National Samples, 1955 and 1980

Subsample	*1955 (N)*		*1980 (N)*[a]		*Change*	*Two-Tailed Significance Level*
Age						
29 and under[b]	44.1	(136)	25.6	(387)	−17.9	<.02
30–49	50.8	(413)	32.1	(577)	−18.7	<.001
50 and over	57.8	(232)	44.9	(323)	−12.9	.061
Occupation						
Professional and business	57.2	(159)	42.8	(449)	−14.4	<.05
Clerical and sales	43.2	(139)	24.9	(193)	−18.3	<.05
Manual	50.0	(420)	27.1	(575)	−22.9	<.001
Farm	65.0	(83)	50.0	(50)	−15.0	.298
Religious preference						
Protestant	50.2	(613)	33.9	(759)	−16.3	<.001
Catholic	44.1	(152)	33.3	(372)	−10.8	.168

[a] Weighted samples. The *N*s for the unweighted samples were used for significance tests.

[b] The lowest age included in this interval is 21 for 1955 and 18 for 1980.

ness persons and was similar to that of clerical and sales workers.[6] This is essentially the same as the pattern of variation in job satisfaction shown by responses to the global job satisfaction question (Quinn et al., 1974: Table 1).

Although the Protestant ethic thesis, as set forth by Max Weber (1930), does not necessarily imply that Protestants and Catholics in contemporary societies should differ in their orientations to work, some authors have believed that there probably are such differences (e.g., Lenski, 1963). In general, however, survey data on work attitudes in the United States have not shown important Protestant-Catholic differences (Schuman, 1971), the 1955 Gallup data used for this study being among the few exceptions (Tables 2 and 3). The Protestant-Catholic difference shown in Table 2 does not approach statistical signifance, but the one in Table 3 does. The two-tailed probability for the latter is .063, and since the difference is in the direction predicted by the modified Protestant ethic thesis, use of a one-tailed test of significance is justified.

In an earlier analysis of these data, Broom and Glenn (1966) showed that the Protestant-Catholic differences in reported enjoyment of work in 1955 were very small in nonsouthern metropolitan areas,

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[6] Although the differences shown in Tables 2 and 3 between manual workers and professional and business workers for 1955 are not significant, according to our estimates, the differences shown for 1980 are both significant (two-tailed $p < .001$ and $< .01$, respectively).

Table 3. Percentage of Full-Time Workers Who Said They Enjoyed Hours on the Job More than Hours Not on the Job, by Selected Demographic and Social Characteristics, U.S. National Samples, 1955 and 1980

Subsample	*1955 (N)*		*1980 (N)*		*Change*	*Two-Tailed Significance Level*
Age						
29 and under[b]	30.1	(136)	24.3	(387)	−5.8	.401
30–49	35.1	(413)	22.4	(577)	−12.7	<.01
50 and over	53.9	(232)	27.2	(323)	−26.7	<.001
Occupation						
Professional and business	43.4	(159)	29.6	(449)	−13.8	<.05
Clerical and sales	35.3	(139)	22.8	(193)	−12.5	.139
Manual	35.2	(420)	16.9	(575)	−18.3	<.001
Farm	61.4	(83)	60.0	(50)	−1.4	.992
Religious preference						
Protestant	40.6	(613)	23.6	(759)	−17.0	<.001
Catholic	28.3	(152)	23.9	(372)	−4.4	.484

[a] Weighted samples. The *N*s for the unweighted samples were used for significance tests.

[b] The lowest age included in this interval is 21 for 1955 and 18 for 1980.

and they speculated that these small differences resulted from differences in age and in origins by region and community size. They implied that high reported enjoyment of work was associated primarily with rural residence and origins and only incidentally with Protestant religious preference—a speculation consistent with the very high level of enjoyment of work reported by farmers in both 1955 and 1980 (Tables 2 and 3). This speculation is also consistent with the data showing a distinct decline in reported enjoyment of work from 1955 to 1980 for Protestants but not for Catholics (so that there were virtually no Protestant-Catholic differences in 1980), since the proportion of persons with rural-farm backgrounds has declined more steeply in recent years among Protestants than among Catholics.[7]

The data by age in Tables 2 and 3 are not amenable to any obvious and clearcut interpretation. The variation in responses by age at each date could be age effects, cohort effects, or compositional effects (or some combination thereof—Glenn, 1977). Stated differently, (1) workers may become inclined to enjoy their work more as they grow older, because many of them find more suitable jobs, lower their expectations, develop higher job proficiency, or change in some other way because of age or experience; (2) each birth cohort which has reached adulthood in recent years may have been less inclined than the ones before it to enjoy work, because of differences in preadult formative experiences and socialization; or (3) workers less inclined to enjoy work may have left the work force in substantial numbers, beginning as they reached middle age, and this attrition may have been due to both retirement and mortality.[8]

---

[7] The contemporary pattern by age reflects the change, and according to combined data from the 1978 and 1980 General Social Surveys, the percentage of Protestants who lived on a farm at age 16 exceeded the comparable percentage of Catholics by 25.8 percentage points at age 50 and older, by 11.8 points at ages 30–49, and by only 8.7 points at ages 18–29.

There is no way to make a definitive choice among the alternative explanations (Glenn, 1976), but some clues can be gained by comparing the 1955 and 1980 responses to the enjoyment of work questions from respondents from about the same birth cohort, as in Table 4. The intracohort comparison is not precise, since the cohort which was ages 25–39 in 1955 was ages 50–64 in 1980, and responses from a few persons age 65 and older are included in the 1980 data. There is little reason, however, to exclude those few responses, since the intracohort comparison is imprecise anyway, due to attrition from the work force of many members of the 15-year birth cohort during the 25-year period.

The striking aspect of the data in Table 4 is the very close correspondence of the 1955 and 1980 responses. So far as we can tell from these data, there were no important age or compositional effects. It is possible, of course, that positive age or compositional effects were almost precisely offset in the cohort by negative period effects during

Table 4. Responses of Full-Time Workers to Questions about Enjoyment of Work, U.S. Samples, Persons Ages 25–39 in 1955 and Age 50 and Older in 1980

	1955 *Ages 25–39*	*1980* *Age 50 and Older*
Do you enjoy your work so much that you have a hard time putting it aside?		
Yes	44.6%	44.9%
No	53.2	53.3
Don't know	2.2	1.9
Total	100.0	100.0
(*N*)	(314)	(323)[a]
Generally speaking, which do you enjoy MORE—the hours you are on your job, or the hours when you are NOT on your job?		
On job	29.0	27.2
Not on job	60.2	57.0
Don't know	10.8	15.8
Total	100.0	100.0
(*N*)	(314)	(323)[a]

[a] Weighted sample.

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[8] We know of no evidence relating mortality to any dimension of job satisfaction, but there is evidence that persons high on some other dimensions of psychological well-being have lower than average mortality (Palmore and Jeffers, 1971).

the 25-year period. However, assuming that the age effects did not change from 1955 to 1980 (i.e., that there were no age-period interactions), age effects should have produced about the same pattern of variation by age at the two dates. In fact, the pattern of variation by age shown in Table 3 is quite different for the two dates, and we can think of no reason why age effects should have changed substantially from 1955 to 1980. Likewise, compositional effects should have been similar at the two dates and should have produced similar patterns of variation by age.

It seems likely, therefore, that the variation by age reflects largely cohort effects and that the overall downward trend in reported enjoyment of work resulted largely from cohort succession—the dying off and retirement of workers from the cohorts with high mean work orientation and their replacement with members of less work-oriented younger cohorts. If so, we speculate that the differences among the cohorts reflect in part the smaller percentage of persons with rural-farm origins in the birth cohorts which recently reached adulthood. According to combined data from the 1978 and 1980 General Social Surveys, only 10.9 percent of persons ages 18–29 lived on a farm at age 16, compared with 17.5 percent of those ages 30–49 and 32.5 percent of those age 50 and older. If this explanation is correct, there may be no substantial decline in the propensity to enjoy work in future cohorts which reach adulthood (since there can be no further substantial reduction in the percentage with farm origins), and indeed the data in Tables 2 and 3 show relatively little difference between the cohorts which were ages 18–29 and 30–49 in 1980. However, there should be an additional moderate reduction in reported enjoyment of work in the total work force as workers who were age 50 and older in 1980 retire and die.

If, as we are inclined to believe, the decline in reported enjoyment of work resulted largely from cohort succession, the decline grew out of changes in the orientations and attitudes workers brought to their jobs rather than out of changes in job conditions and in the nature of work. However, our "cohort succession" explanation is quite tentative, and we do not wish to discourage attempts to find recent changes in conditions of work which may have tended to lessen work enjoyment.

Implications

Much of the research on job satisfaction and attitudes toward work has been motivated by a desire to improve job performance and productivity. However, the relationship between reported global job satisfaction and job performance seems to be quite tenuous, at best (see Berg et al., 1978: chap. 3, for an excellent summary of the evidence). The relationship of enjoyment of work, as it is measured here, to job performance is not known, but one might guess that it is more strongly positive than is the relationship of many other dimensions of job satisfaction to job performance. Research on the relationship between work enjoyment and job performance is needed, and until it is performed, one cannot even begin to estimate what impact, if any, the apparent decline in enjoyment of work has had on productivity. Although we share the widespread view that the primary bases of productivity are structural and technological, we believe that it is a mistake to assume that the psychology of individual workers is not important as well. And if, as we suspect, enjoyment of work is affected to an important degree by the attitudes and orientations workers bring to the job, it is not merely a reflection of structural conditions.

Aside from considerations of productivity and job performamce, the data reported in this paper are important indicators of the "quality of life." Since a large proportion of adult Americans spend around a third of their awake hours on the job, enjoyment of work is desirable for its own sake, even if it does not enhance job performance. The decline in the percentage of persons who said they enjoy hours on the job more than hours not on the job is not necessarily a negative indicator of quality of life, since the change could have resulted from increased enjoyment of nonwork activities rather than from decreased enjoyment of work. (In either event, the change indicates an increase in a leisure-oriented way of life.) However, the decrease in the percentage of persons who said they enjoyed their work so much they had a hard time putting it aside reflects either a real decline in the percentage of persons with very high enjoyment of work or a decline in a tendency to pretend to enjoy work intensely. Although a very high level of work enjoyment is probably not essential for a very high level of general psychological well-being, any decline in the percentage of workers who intensely enjoy their work constitutes an important decrement in the quality of American life. Research to investigate the sources of this apparent decrement should be conducted without undue delay.

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Television and American Culture: The Mass Medium and the Pluralist Audience

W. RUSSELL NEUMAN

TELEVISION has emerged as the truly dominant mass medium in American society. The numbers are staggering. Ninety-eight percent of American homes have sets and those sets are turned on for an average of about seven hours a day. The average adult takes in an incredible four hours of television a day, which over the average lifetime adds up to the equivalent of eight straight years (at 24 hours a day) of television viewing. Viewing has been rising steadily and significantly among all age, educational, and ethnic categories over the last decade (Comstock et al., 1978).

Moreover, advertisers invested some 12 billion dollars in 1980 in order to present their commercial messages before these teeming multitudes. These large-scale economics themselves become a factor in the character of the medium as competition among the networks becomes increasingly intense for a larger share of that considerable pie. Programs are designed to have the broadest possible appeal because if even a small segment of the viewing audience is unamused (not to mention potentially offended) a resulting loss of even a single

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Abstract The study explores the potential influence of television on trends toward cultural homogenization in American society. Depth interviews focused on two variables: the analytic response—viewers' thoughts about the program itself, including, for example, comments on how plot elements relate to the program as a whole or the character of the script or acting, and the interpretive response—viewers' thoughts about the program's relevance to their own lives or broader issues of society and culture. The data support the hypothesis of cultural homogenization revealing similar indices of analytic and interpretive response across educational levels.

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ratings point for one program over the course of a year amounts to a potential 15-million-dollar loss in revenues for the network.

Perhaps the most striking aspect of the television phenomenon from a sociological point of view is its universality. Television is socially defined as the culture of the masses. And although professors, professionals, and the educated elite in general may claim to have time only for occasional news, sports, and a play or concert on the public channel, the recurrent implication of systematic research is that they watch the same situation comedies and action adventure programs just about as often as their neighbors in blue collars and hard hats (Wilensky, 1965). As Hirsch has put it, television has come to provide a "centrally produced, standardized and homogenous culture" (1978, 400). Social scientists, however, have devoted surprisingly little attention to the long-term impact of this core of shared experience in our society.

To what extent has television become a major force of social integration and cultural uniformity? Are individuals from diverse social and cultural backgrounds really responding in the same ways to these common cultural forms? We find that better educated viewers watch slightly less television than the less well educated—about three minutes less per day for each additional year of formal education—but the types of programs viewed are virtually identical.[1] Are better educated viewers more attentive and do they draw on extensive exposure to literature, theater, and cinema in responding to the television they see?

In an attempt to explore whether people from diverse backgrounds who *watch* the same program actually *see* the same program, we designed a depth interview technique based on a series of increasingly structured questions about which elements in a program stand out in viewers' minds and how they evaluate what they have seen. Transcripts of these interviews were systematically coded to derive quantitative measures of the attentiveness as well as the level of analytic and interpretive thinking among various social groups in response to a random sample of prime-time television programming. The analysis focuses on four questions: (1) Is television thought-provoking? (2) Is the response to television differentiated by educational level? (3) Does the level of response vary across program types, or was McLuhan right after all, that the medium pretty much determines the message? (4) To what extent do the expectations and attitudes of the viewer (the "social definition of television") influence the medium's impact?

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[1] The three-minutes-per-day figure is derived from Bower (1973), Table 6–2, p. 132. Some studies have suggested that longer working hours and more out-of-house social activities rather than cultural tastes explain the small negative relationship between education and television exposure (Samuelson, et al., 1963).

Research Design

No single study could possibly determine to what extent television programming is perceived as homogeneous or differentiated. Various research designs, emphasizing one or another dimension of response, or different types of programming, might plausibly reach divergent conclusions. Our strategy was simply to plunge ahead and derive some initial comparisons based on a measure of the extent to which programming is thought-provoking.

A total of 30 programs representative of commercial and public offerings during prime time, ranging from a half-hour to three hours in length, were randomly sampled during April 1979. Twenty programs were sampled from the three commercial networks, representing roughly a 29 percent sample of the 70 programs they provide each week in prime time. Ten programs were sampled from PBS, representing roughly 36 percent of the weekly prime-time offerings.

Three groups of television viewers were interviewed by telephone shortly after watching one of the sampled programs. The first group was a random sample of southern Connecticut residents drawn from the city directories for New Haven County—urban, suburban, and suburban fringe areas characteristic of the industrial Northeast. The region is ethnically diverse, with large Italian and black communities. Census data indicate that both the mean and standard deviation for level of education in this area are equivalent to those of the country as a whole. Respondents were initially contacted by letter. Interviewers then phoned to solicit participation, asking respondents to watch a particular program the following evening. Seventy percent of the mass sample agreed to be interviewed, which, given the complexity of the procedure and length of the interview, seemed to be an acceptable level.[2]

The second group was obtained by the following procedure. Interviewers called households at random until they located an adult who had just seen one of the television programs used in the study. The advantage of this procedure is that the respondents had no knowledge that they would be interviewed about a program until after they had seen it under normal home-viewing conditions. Such a sample, of course, generalizes to television viewers rather than community residents.

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[2] Determining the response rate for the ad hoc viewer group would require a different set of calculations. Most of the households called did not contain viewers of the specific program in question and were simply passed over. Estimating the appropriate response rate would require local television ratings data, which are not available for this time period.

A third sample was drawn from the humanities faculty at four area liberal arts colleges. This special professorial "control" group allows us to explore the importance of advanced training in the humanities for interpretive skills with this medium. Fifty-five depth interviews were completed with the mass sample, 24 with the special ad hoc television viewer group, and 34 with the humanities scholars. The response rate for the scholars, who were recruited in the same way as the mass sample, was 80 percent (surprisingly, only two of the professors we called did not have television sets).

The interview itself, immediately following the program, was conducted by phone and tape recorded (with the permission of the respondent) to allow for further analysis and coding at a later time. The interview took from 20 to 40 minutes to complete and involved 31 question-probes. In the first section of the questionnaire the four questions were as broad and nondirective as possible, asking what thoughts and emotions the program brought to the respondent's mind, and what elements of the program were liked and disliked. A second section of the interview included six recall questions focusing on prominent characters, the plot, and the script. A third section involved 21 dimensions of program evaluation, including thoughts about the plot; pace, genre, and technical aspects of the program; as well as speculation on the scriptwriters' intentions, the program's relevance to the viewer's personal life, and potential social and political implications of the program.[3]

Obviously, viewers may be daydreaming or simply thinking of matters entirely unrelated to program content, so we narrowed our inquiry to two types of cognitive response—thinking about the program itself, or relating the content of the program to one's own life or broader issues for the community or society. We identify the former as *analytic thought*—taking the program apart in one's mind, examining how the pieces relate to each other and to the program as a whole, and evaluating the plot, pace, script, acting, or technical elements of the production. We identify the latter as *interpretive thought*—the consideration of what social, cultural, or organizational factors might have influenced the writers and producers of the program, and in turn how the program might influence the thinking of a typical viewer, as well as the respondent's own sense of the broader meaning, if any, of the program's themes or their relevance to his or her personal situation.

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[3] The full interview protocol and a description of coding procedures are available from the author.

After the interviewing was completed, a separate staff of six carefully trained coders combed through these interview tapes, counting each analytic or interpretive comment made by the respondents. Comments might have been made in response to a general query about what was liked or disliked in the program or in response to a more specific question about the script or acting. Many respondents tended to repeat themselves in the course of the interview; in such cases each unique analytic or interpretive comment was coded only once. Vague evaluative statements such as "I liked it," or "It was crummy," which did not relate the program or some specific element within it to a more clearly defined evaluative dimension, were not coded. In each case interviewers probed respondents to explain why they liked it or what they meant by "crummy." Thus, such statements as: "the plot was contrived," "they did a good job of developing that woman's character," or "the dialogue in the romantic scenes at the end was rather wooden" would be coded as an analytic observation. Such comments as "that program is aimed at a younger audience," "the networks just pay attention to ratings," "that's a weird portrayal of justice, police departments just don't work like that," or "it makes you think about the level of violence in our society" would be coded as interpretive comments. Coding decisions about the presence of analytic or interpretive comments turned out to be fairly straightforward. Each of the coders was assigned to code an identical subset of interviews independently. The intercoder reliability was calculated at .79 (Pearson).

The basic strategy, then, was simply to give respondents an extended opportunity to describe how they reacted to a cultural work, in this case a randomly selected television program. We computed indices of which programs were better liked, but rather than make an evaluation per se we concentrated on which elements of the various programs were most salient for respondents of different educational levels. A less articulate respondent, of course, would tend to score lower on these measures. Perhaps some individuals did have a sophisticated and thoughtful response to a program but because they are unaccustomed to dealing with such matters, were less able to articulate their ideas in the course of the interview. But the technique of coding open-ended interviews rather than using the more traditional precoded survey items, and the fact that respondents were given ample time and encouragement to simply express whatever thoughts came to mind, would indicate a sensitivity and flexibility in the data gathering appropriate to the hypotheses under study.

Is Television Thought-Provoking?

A thoughtless, passive state did seem to characterize some viewers. Twenty-five percent of our respondents, for example, had no substantive response at all to the first section of the interview. They could recall no thoughts which had come to mind while viewing. They could recall no emotional reactions. And they could not identify any element of the program which they either liked or disliked.

Some respondents were well aware of their passivity and revealed no particular embarrassment about it. One government worker, asked what thoughts had come to mind, responded, "Nothing, really . . . my mind was quite blank." A mechanic in response to the same question mentioned, "A show like this gives me a chance to rest my mind and think about other things." A department store sales clerk: "What thoughts, nothing, nothing really, I was just waiting, you know, waiting for the comedy to happen." But it would be a mistake to interpret this as evidence that their minds were in fact blank or entirely inattentive to events on the television screen. Perhaps a more appropriate conclusion would be that the ephemeral character and low salience of their thinking makes it hard to recall. Also some respondents, especially at the outset of the interview, may simply have felt that despite our insistent questioning, their random thoughts were not worth mentioning. It is important to emphasize, however, that a great deal of variation in viewer response was evident, depending both on the character of the program and the orientation of the viewer. For example, one viewer, a 50-year old assembly-line worker, in response to the first four questions, reeled off eight issues he had been mulling over in his mind while watching a made-for-TV movie. Among other issues, he commented spontaneously on its impact on the viewing audience, how it reflected changes in moral standards, and the quality of the acting and the script. In response to these first four queries, the average respondent made 2.2 comments.

The most frequently mentioned aspect of the sampled programs in this section of the interview was the quality of the acting and the relevance of the program for the personal life of the viewers (each noted by 26 percent of the respondents), followed by comments on the plot, 24 percent, the realism (or lack thereof) of the program, 22 percent, comparisons and contrasts with other television programs, 18 percent, and social problems raised by the program, 13 percent. A variety of other comments were made but in each case by less than 10 percent of the respondents. Positive or approving comments outnumbered negative ones two to one. These percentages, however, are averaged from the 30 programs we sampled and could change significantly as programming trends evolve in the television industry. Typically, a comment would be both brief and straightforward, noting, for example, that the plot dragged a bit in the middle, or that a supporting actor seemed particularly talented, or that a daughter-in-law had a problem similar to that of the heroine. Such observations seldom delved very deeply into the nature of drama or comedy.

The initial four open-ended queries have the advantage of being nondirective and provide an idea of which dimensions were most salient for these respondents. But they have the disadvantage of generating a very incomplete measure of the nature and variety of responses in the audience (Schuman and Presser, 1979). Thus, because people may not easily remember details of their initial reaction to the program, we pursued two further levels of stimulated recall in the interview. First, interviewers mentioned two randomly selected characters, two plot elements, and two segments of dialogue, and asked in each case how the viewer responded to this element in the program. Next, interviewers ran down a list of 21 questions probing for analytic and interpretive responses to the program as a whole.

In response to the total of 31 queries about each program, the average viewer made 7.8 unique and nonoverlapping comments.[4] Almost all of the probes generated a meaningful response from a substantial number of viewers. The probes most often responded to involved plot organization and the intent of the authors, each of which stimulated comments in 55 percent of the interviews. The probes with the least response involved political ramifications of programs (16 percent), and comments on symbolic elements (8 percent).

As already noted, there is evidence of considerable variation on these measures, across both programs and people. The coders found as few as 3 and as many as 20 analytic or interpretive comments among mass sample viewers in responding to the same program. Some programs stimulated an *average* of nine times as many responses among randomly assigned viewers as the least thought-provoking programs.[5] We turn next to an attempt to partition this variance and explore to what extent a thoughtful response is due to the character of the viewer rather than to the quality of the program.

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[4] Of this total, 4.4 were categorized as analytic and 3.4 as interpretive comments.

[5] Samples for individual programs ranging from three to six interviews are much too small to justify a detailed analysis.

Is the Response to Television Differentiated by Education?

In this section we will examine three related hypotheses about whether class-linked differences of perception might work for or against the potentially homogenizing influences of television on American culture.

The Cultural Experience Hypothesis predicts that education will have a positive correlation with indices of analytic and interpretive thinking. Although the better educated may claim that they do not particularly enjoy watching television, when they do watch it is probably with moderate attentiveness. Perhaps they cannot help noticing techniques of plot development or comedic devices or visual symbols as they would with a motion picture or play. From their more extensive experience with "serious" art, the better educated have developed skills in interpreting themes and in speculating on the relevance of narratives for their personal life and for society as a whole.

This hypothesis suggests further that television may occupy a somewhat different role for those who did not finish high school or go to college. Because of limited exposure to either traditional aesthetic values or more technical issues of the various forms of literary analysis, these viewers may be neither inclined nor able to treat television as anything more than diverting entertainment, a pleasant escape from the burdens of the day.

The Cultural Polarization Hypothesis predicts just the opposite pattern—a negative correlation between analytic/interpretive indices and educational level. The idea in this case is simply that television is itself the prime cultural enterprise of the masses. Presumably, television functions for the less educated as plays, books, and the serious arts do for the better educated. Perhaps the less educated viewer watches more attentively, searching for broader meanings and lessons for his or her personal life, while better educated viewers, by defining television as strictly entertainment, ignore interpretive questions and treat TV as pure escapism.

Finally, the Homogenization Hypothesis suggests yet another possibility. It may be that television functions as a common cultural form and is appraised in very similar ways by different educational groups. The prediction here would be no significant differences in analytic and interpretive scores across educational levels. Both the assembly-line worker and investment counselor, it is suggested, come home in the evening weary from the day's labors and seek relaxation and light entertainment. Neither approaches television with the intent of being informed or having their cultural horizons broadened, but both inevitably find themselves to some extent evaluating and thinking about the programs they watch, contrasting ideas found there with their own lives and other cultural experiences.

If the Cultural Experience Hypothesis is supported by the data, one's attention is drawn away from the character of television per se and toward what it is in the educational process which makes audience members attentive and thoughtful. In contrast, support for the Cultural Polarization Hypothesis would draw attention toward the character of television programming. If one stratum of society is relying on television while another relies on print media for information, a reinforcement of values, and a sense of their place in the world, then the unique characteristics and emphases of television news and drama could potentially lead to a class-linked polarization of world views.[6] The third hypothesis is most like the second in drawing attention to how television may be influencing attitudes. In this case, however, all educational levels would be equally subject to the allure and accumulative powers of gentle persuasion in television's definition of reality.

Table 1 contains data for the critical test—an analysis of variance in analytic and interpretive scores by education. Of course, given this small sample of television viewers, we can take only a first step toward identifying which of the three hypotheses is more nearly correct. But as is often the case in empirical work of this sort, the data raise several further issues and suggest a reconsideration of the original assumptions. The original three hypotheses anticipated either a positive or negative linear relationship or no relationship at all. Table 1 apparently demonstrates a nonlinear pattern—very little difference in response to television among different educational levels in the mass sample but a distinctly higher level of response among the scholars. One might have expected the humanities professors to be particularly hostile toward television and, as a result, disinclined in principle to respond analytically or interpretively. But the data reveal that the professors were rather open to using their skills and experience in evaluating the television programming they saw. Perhaps humanities training emphasizes analytic rather than interpretive skills,

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[6] The ramifications of this polarization scenario have been reviewed by Tichenor and his colleagues, 1970.

Table 1. Education and the Response to Television: Average Number of Comments per Depth Interview

	Education					F/Sig.	
	Less than H.S.	H.S.	Some Coll.	Coll.	Scholars	4-way	5-way
Analytic response	(15)	(39)	(18)	(17)	(34)	(89)	(123)
General form (plot organization, pace, character development)	1.42	1.13	1.11	1.75	3.54	.7 n.s.	10.1 <.01
Generic form (comparison with other television programs in same series or genre, or comparison with other media—theater, film, books, etc.)	1.07	1.13	.64	1.43	2.38	1.4 n.s.	6.4 <.01
Technical form (script, formula, symbols, acting, production, editing, etc.)	.84	2.43	1.96	1.86	6.53	3.7 <.01	18.4 <.01
Total analytic	3.31	4.68	3.70	5.03	12.44	.9 n.s.	14.7 <.01
Interpretive response							
Intent (ideas or feelings authors intended to arouse in audience, motivations of producers, television executives)	.92	1.11	.76	1.43	1.86	1.6 n.s.	5.4 <.01
Impact (impact of program on own thinking or emotions, speculations on impact on average viewer)	.71	1.30	.69	1.37	1.50	1.9 n.s.	1.7 n.s.
Implications (broader meanings or implications for society, current social problems, or political, moral or religious ramifications)	1.27	.96	.98	1.92	2.38	2.0 n.s.	4.4 <.01
Total interpretive	2.90	3.37	2.43	4.72	5.74	2.3 n.s.	3.6 <.01

since their score on the former is dramatically higher than the mass audience and on the latter it is less so. Yet both are statistically significant. Nonetheless, whatever it is that the scholars learned in graduate school and their professional careers has apparently not been successfully passed on to their students. The number of analytic and interpretive comments among the college-educated in the mass sample is just slightly higher (and on one dimension, technical form, slightly lower) than among those with less education.

One possible interpretation of these findings is that education may be weakly related to a more analytic and interpretive response to ideas communicated via television—a gently sloping positive relationship for a mass population the precise parameters of which could be estimated with a much larger sample size. But because the scholars represent an extreme educational elite, the differences are conspicuously evident even with small samples and considerable measurement error. Given such an interpretation, the evidence provides support for both the Cultural Experience and the Homogenization Hypotheses, because although educational exposure makes a difference, it is so small across the range of natural variation in the mass population that television in practical terms serves as a cultural leveler. This leads one to question whether educational differences in response to books, periodicals, and newspapers might be much greater. Is the lack of class-linked difference in response to television perhaps the result of television's overwhelming emphasis on mass appeal entertainment? We turn to that issue next.

The Commercial Networks Versus Public Broadcasting

Since 1967 the public broadcasting system in the U.S. has made available a fairly complete schedule of programming focusing on public affairs, drama, and the performing arts traditionally associated with high culture and for the most part previously available only in live performance or the print media. Of course, it was recognized that those already predisposed toward classical music, science, and Elizabethan drama would be the first to tune in and that, to some extent, public broadcasting would be providing specialized cultural programming for a rather small special-interest group.

In this case, two alternative hypotheses guide the analysis. First, the Elite Culture Hypothesis follows the traditional definition of educational and public television and predicts that the public programming on the average will provoke a more analytic and interpretive response than commercial television and that this will be true for both the mass sample and the scholarly sample. The better educated might find the content of public television a bit more familiar and more enjoyable and would, perhaps, respond with a special articulateness and enthusiasm. But the Elite Culture Hypothesis emphasizes that the primary source of variation in analytic and interpretive response is the character of the program rather than the background and orientation of the audience member. Accordingly, even those not broadly read in the classics are expected to find Shakespeare more thought-provoking than the typical commercial situation comedy.

Second, the Taste Culture Hypothesis predicts that scholars will find public television more thought-provoking and those with less education may be either anxious and confused, or simply bored, and will have relatively little to say about the characters, themes, and information presented on the public channel. Herbert Gans (1974) has developed a notion of educationally linked taste cultures, which emphasizes that the familiarity with higher cultural forms which comes naturally from the educational process is a critical element. Tastes have to be cultivated over time and although a diversity of widely available and inexpensive cultural forms is desirable, it is naive to expect that availability alone will lead those unfamiliar with the arts to take to them quickly. Since we know that the scholars in the sample watch little television and are relatively unfamiliar with normal commercial fare, the Taste Culture Hypothesis predicts that scholars will exhibit higher scores on the analytic and interpretive indices for public television, and the mass sample will respond with higher scores for the commercial programming.

First, we contrasted the average scores for both the mass sample and the scholar sample for all programs. Next, because situation comedies and action-adventures represent genres seldom if ever present in the public television schedule, we recomputed the comparison including only dramatic and public affairs programs on commercial and public television.

In both cases, the data tend to support the Taste Culture Hypothesis. The depth interviews reveal roughly twice as many analytic comments from the mass sample in response to the commercial programs. The scholars, in contrast, found public television more analytically provocative. On the interpretive dimension, we find a less dramatic difference, but again the mass public is more responsive to commercial fare while the scholars are more responsive to public programming. Surprisingly, these patterns are even stronger when the comparison is limited to public affairs and dramatic programs.

This is not evidence of a complete cultural polarization. Mass audiences are not entirely unresponsive to the types of dramatic and public affairs programming public television provides. In fact, we asked respondents to rate on a scale from zero to 10 how much they enjoyed viewing the assigned programs, and averages for the commercial and public programs were indistinguishable for the mass sample (the scholars said they enjoyed the public programs more).[7] Also,

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[7] On this scale, zero indicated the least favorable and 10 the most favorable evalua-

Table 2. Responses to Alternative Programming: Average Number of Comments per Depth Interview

	Analytic Comments				*Interpretive Comments*			
	Comm. TV	*Public TV*	*T*	*Sig. Level*	*Comm. TV*	*Public TV*	*T*	*Sig. Level*
All programs								
Mass sample (89)	5.51	2.89	10.3	<.01	3.76	3.21	0.7	n.s.
Scholar sample (34)	11.77	14.22	1.3	n.s.	5.23	7.11	1.9	n.s.
Public affairs and drama programs only								
Mass sample (41)	6.13	2.89	13.8	<.01	4.72	3.21	3.9	<.05
Scholar sample (19)	10.50	14.22	1.9	n.s.	6.10	7.11	0.3	n.s.

some of the difference may be a result not of an unthoughtful response from the mass sample but of a reluctance to comment for fear of giving the "wrong" answer.[8] Further research into the types of comments made and into the variation in mass response to several types of public programming and to print media will help to clarify the nature of this polarization. The available evidence, however, demonstrates that there is more to the message than just the medium, that the type of programming does indeed make a difference. But, contrary to the usual assumptions about the thought-provoking character of the serious arts, we find that the American mass audience finds commercial television distinctly more thought-provoking. This raises some interesting questions about the appropriateness of current strategies and the ultimate goals of public broadcasting, and whether the lack of resonance with the mass audience is due primarily to a general skepticism about the importance of elite culture, or to an incompatibility of those cultural forms with the small electronic screen.

The Social Definition of Television

Many people, when they are in the mood to watch a little television, simply turn on the set, find the least objectionable program, and then sit back to watch. Others carefully review the program guides

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tion of a specific program. The mass sample rated both commercial and public programs an average of 6 on the scale. The scholars rated commercial programs at about 4 and public programs at about 7 on this scale.

[8] The actual interview transcripts, however, reveal little evidence that respondents were concerned about giving "wrong" answers or that they were hesitant to express evaluations which might have differed from interviewers' opinions.

and television critics, discuss programs with friends, watch more selectively, and generally define the medium differently. Exploring such differences in attitude and context of viewing may help to fill in the gaps in our understanding of variation in audience response.

We developed an index of orientation toward television comprised of three intercorrelated variables: (1) evaluation of television as a potentially rewarding leisure activity, (2) the extent to which specific programs are discussed with friends and colleagues, and (3) the extent to which viewing is scheduled through the use of program listings. The index was designed to separate the casual, low salience, least-objectionable-program viewer from those who are more active and selective. If such attitudes make a difference, we would expect the index of a positive and active viewing orientation to be associated with higher analytic and interpretive scores.

A related issue is the context of viewing. The orientation index attempts to measure the general predisposition of the viewer toward the medium. But it is likely that moods and motivations may vary over time for a given individual—who may sometimes actively seek out a program of interest and other times may turn on a late-night movie as an aid in getting to sleep. Two variables in our field experimental design provided some leverage for exploring the immediate context of viewing. First, part of the mass sample had been contacted in advance and asked to watch a specific program. Those in the second mass sample group were interviewed if they happened to have seen a program under normal home viewing conditions. The second group, of course, had no foreknowledge of the interview. If indeed the immediate context and expectations of the viewer make a difference, it should be evident in a comparison of the average scores for the scheduled and ad hoc mass samples.

A second context variable arises because we completed two interviews on two separate programs with most of the scheduled viewers. Since respondents were familiar with the questions and probes in the second interview, we expected to find significantly greater numbers of analytic and interpretive comments.

The results of analyses involving these three variables are shown in Tables 3 and 4. As can be seen from Table 3, the orientation of viewers does make a difference, but the correlation is in the opposite direction. Those who were oriented toward selective viewing turned out to be the least responsive to the programs they saw.

There were further surprises on the context variables. Table 4 indicates that prearranged interviews did generate somewhat higher scores, but the differences were not very great. Perhaps most surprising was the lack of a sensitization effect on the results from the

Table 3. The Social Definition of Television: Average Number of Comments per Depth Interview

Positive Orientation Toward Television [Mass Sample Only]	*Analytic Comments*	*Interpretative Comments*
Low (21)	5.09	4.86
Medium (37)	4.27	3.16
High (27)	4.03	2.67
	$F = 0.7$/n.s.	$F = 5.7/<.01$

second interview. Program assignments were randomized, so both first and second interviews dealt with equivalent program types. The numbers of analytic and interpretive comments were not significantly different in the second interview and the averages were actually slightly lower than the first. Since interviews were scheduled a week apart, it is unlikely that a test fatigue phenomenon of the usual sort was in evidence here. We concluded simply that sensitization was not a major factor and that considering the small difference due to prearrangement, the depth-interview procedure is robust and generates data which are not significantly distorted by context cues.

A substantive conclusion which might be drawn from the context data parallels the earlier finding of minimal educational differences in the mass sample. We refer to the apparent ceiling effect in the number of comments television programs tend to stimulate among viewers. Whether or not an interview was prearranged, whether or not a previous interview had been conducted, and whether or not the respondent has a college education, the level of response is very close to that of the sample as a whole. The single exception was the special scholar sample, whose self-selection, extensive training, and professional experience characterize it as a truly unusual and atypical subgroup of viewers.

As with the earlier finding of minimal educational differences, these patterns run contrary to the traditional research literature concerning communication effects, which documents significant influences of

Table 4. The Context of Viewing: Average Number of Comments per Depth Interview

	Analytic Comments	*Interpretive Comments*
Prearranged interview (65)	4.71	3.59
Ad hoc interview (24)	3.57	2.96
	$T = 2.2$/n.s.	$T = 1.1$/n.s.
First interview (34)	8.32	4.29
Second interview (34)	6.82	4.18
	$T = 1.3$/n.s.	$T = 0.1$/n.s.

both education (Weiss, 1969) and communication context variables (McGuire, 1969). Since the great majority of studies of learning and persuasion involved either print media or lecture-presentation experiments, one possibility is that the established wisdom about which variables influence communicative effectiveness does not apply to communication through television. Perhaps until either the print orientation of the educational system or the social definition of television changes, the effect of this medium will be unique.

Directions for Further Research

A central concern of this analysis was the prospect of cultural homogenization through television. We have focused here primarily on education as the stratification measure, and find relatively small differences among the different strata in responses to the ideas and information presented on commercial and public television. Since for many years survey research has revealed very distinct differences in attitude and information level across educational groups (Hyman et al., 1975), we take the lack of differentiated response in this case as possible evidence of a cultural leveling phenomenon.

These results raise interesting questions about the effect of formal education on patterns of information seeking and cognitive styles in processing information and cultural materials. Because high school students are repeatedly challenged to appraise, evaluate, and contrast ideas and information in examinations and paper assignments, there was a strong presumption that this experience would lead to a much more articulate and thoughtful response among the more educated television viewers. Apparently, the heavy reliance in education on traditional print resources has resulted in an educated public lacking either the disposition or skills (or perhaps both) to evaluate television programming critically. Given these findings, special care should perhaps be taken by those in public broadcasting to anticipate how mass audiences will respond to both classical and modern dramatic presentations. Adapting cultural works for television may prove to be as difficult as translating poetry from one language and culture to another, perhaps more so.[9]

[9] Some cynical observers might argue that even a generous supply of well-produced, serious programming would not provoke much in the way of a thoughtful response from the typical member of the American mass audience. This observation harks back to the intriguing question of whether the thoughtfulness of response is due to the sophistication of the viewer or the sophistication of the programming itself. Because two separate interviews on different programs were conducted with most of the scheduled sample, we were able to derive a rough answer to that question. The two interviews were correlated with each other at .46. This could be interpreted as a test-retest correlation

These findings emphasize that although our initial assumptions about which types of programs would be most thought-provoking in the mass audience were incorrect, the character of programming does indeed make a great deal of difference. The character of information being presented, the technology of communications, and the predispositions and cognitive styles of the audience interact in subtle ways. We are just beginning to understand the nature of these interactions.

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indicating that 21 percent of the variance is explained by the sophistication of the viewer and the remaining 79 percent is explained by thought-provoking character of program and measurement error. These are rough estimates, of course. A much better estimate would be derived from a broader sampling of materials and the inclusion of careful estimates of measurement error.

Public Hearings and Public Preferences: The Case of the White House Conference on Families

MICHAEL J. O'NEIL

CALLS for "public hearings" in part represent an attempt to replicate some of the important features of town meetings, although both classic democratic theorists and their elitist critics agree at least on the need for representative, as opposed to direct,[1] democracy. Public hearings differ from town meetings in that the obligations of elected officials to

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[1] The only visible contemporary advocates of direct democracy appear to be those identified with the "public choice" perspective. Their journal, *Public Choice*, contains a variety of proposals for implementing various strategies of direct democracy (e.g., Shubik, 1970; Miller, 1969; Mueller, Tollison and Willett, 1972). Though this group is highly committed, most theorists regard any form of direct democracy as impractical and many think it undesirable. Thus, the main focus of political discourse has dealt with the extent to which constituent preferences should be followed within the context of representative democracy. See Fairlie (1940a, 1940b) for an extensive review of most of the major theories of political representation.

Abstract To provide input into Arizona's participation in the White House Conference on Families, the Arizona Governor's Council on Children, Youth, and Families commissioned a random statewide survey to assess the relative priority given to 41 selected family-related needs and preferences for institutional responses to those needs. A similar survey was administered to participants at each of six regional public hearings held throughout the state prior to the 1980 White House Conference on Families. A comparison of the two surveys provides an opportunity to test the representativeness of public hearings participants with respect to the population from which they were drawn. Fundamental differences in the priorities of these two samples cast considerable doubt on the assumption that public hearings are an effective means of gauging public sentiment.

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carry out preferences expressed at public hearings are unclear. Classic democratic theorists hold that officials act merely as agents of the public and that their function is limited to implementing the public will after informed public debate. It therefore follows that they would favor adherence to the expressions of public hearings—if, in fact, the hearings are representative of the broader public. Elitist democratic theorists, questioning several of the assumptions of classic democratic theorists (Berelson et al., 1954; Walker, 1966), would argue that no such obligation exists,[2] as would Burkean democrats, since Burke urged that elected officials serve their constituents' *interests* but not necessarily their *will* (Miller and Stokes, 1963). The extent to which one is likely to hold that expressions of preference at public hearings ought to be followed is therefore dependent on both a theoretical stance and empirical data pertaining to the representativeness of a particular hearing.

The White House Conference on Families Public Participation Hearings

The White House Conference on Families was actually three separate meetings held in three different cities during the summer of 1980. Prior to these meetings, individual state delegations were charged with devising means for the expression of public sentiment in their respective states. One of the means used in Arizona was the staging of six regional hearings held at different locations throughout the state during February and March of 1980. A second was the commissioning of a public opinion survey to assess issues of concern to Arizona families. The author was principal investigator of this survey, which was conducted between March 24 and March 29, 1980.[3] Because comparable data were collected from these two sources, we are provided with a unique opportunity to examine the representativeness of the public hearings process for this case. Because this is a case study, it is important to put this comparison in a broader context. Therefore, we should first consider the features of this particular case along several dimensions.

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[2] An extension of the elitist reasoning might well maintain that meeting participants are likely to be drawn from the more active, informed, and involved members of the community and that their opinions therefore ought to be given more weight. The most logical response to this argument would warn of the danger that such meetings could be captured by special interest groups whose greater-awareness might be overshadowed by a lower level of concern about overall community welfare. In practice, whether participants at a particular hearing are seen as an enlightened elite or a narrow private interest is likely to depend on whether one agrees with the group's objectives.

[3] A 147-page technical report was submitted by the author to the Arizona Governor's Council on Children, Youth, and Families which was responsible for the initiation and coordination of this survey for the Arizona White House Conference on Families Steering Committee. It contains an analysis of the random statewide survey, the public hearings survey, and a survey of selected community groups. This report may be obtained on request by writing the Governor's Council on Children, Youth, and Families, Post Office Box 6123-012C, Phoenix, Arizona 85005.

THE NATURE OF THE HEARING

Special interest groups seem more likely to dominate single issue meetings than general purpose meetings (Ashford, 1972). Special interests are also more likely to be overrepresented in instances where small groups with much at stake are pitted against larger, more peripherally affected groups (Tucker and Zeigler, 1978). In this case, the hearings were confined to issues relating to the family, but were not otherwise constrained. Thus, topics discussed ranged from abortion, birth control, welfare policy, and equal employment to anything participants considered to be related to family needs.

THE OPINIONS OF PARTICIPANTS BEFORE AND AFTER THE MEETINGS

Differences between the opinions of participants in a public hearing and those of the general public need not be indicative of unrepresentativeness. Rather, they might arise from information acquired during the hearings themselves. Although we do not have the "pre" and "post" data on hearings participants' attitudes that would permit an experimental answer to this question, the contentious nature of these hearings suggests that few opinions were changed as a result of participation. The hearings became battlegrounds for committed partisans rather than opportunities for thoughtful, open-minded citizens to weigh evidence dispassionately. With far fewer than one percent of the state's population participating in any of the hearings, the expressions at them reflected the ability of various groups to mobilize supporters more than the ability to persuade others. Nor is it likely that the hearings influenced the opinions of the general public in the state to any significant degree. Although a few newspaper articles about the hearings appeared durring this period, mostly noting their contentious character, there was little evidence that would indicate a shift in public opinion immediately following the regional state hearings.

DEMOGRAPHIC DIFFERENCES

Demographic differences between participants and the overall population might indicate economic obstacles to participation, differences in political efficacy, or an organized campaign to mobilize supportive groups. In the analysis that follows, we will examine these possibilities as well as the attitudinal differences exhibited between the hearings participants and the random statewide sample.

Data Source I: The Random Arizona Survey

To help inform the Arizona delegates to the White House Conference on Families about citizen priorities, a general population telephone survey of the state was commissioned. The sample was selected by random digit dialing, and trained, closely supervised interviewers completed 673 interviews with a response rate of 80 percent. The traditional estimate of the sampling error at the 95 percent confidence level under these conditions would be ±3.8 percent.

We asked two basic substantive questions about each of 41 separate areas chosen to represent a broad range of family-related concerns:

> How concerned would you say you are about ——? Would you say you are extremely concerned, moderately concerned, only slightly concerned, or not at all concerned?
>
> Who do you think should have the major responsibility for assisting families who need help in this area? If families need outside help, should this be provided by churches, community organizations, private businesses, state and local governments, or the federal government?

The list of "concern" questions was necessarily selective and judgmental. Moreover, "individual families" was a response deliberately omitted from the followup question. The purpose of this question was to determine which outside agencies' assistance would be most preferred *in those cases* where individual family needs could not be met adequately without some external assistance. The question was not intended as a measure of the preference for individual family solutions to family problems. Individual family solutions were *assumed* to be the first preference of most citizens for most of the issue areas covered.[4]

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[4] This "assumption" was a political requirement of the Governor's Council, which sponsored the survey. The council's staff were under intense pressure to avoid any posture that might make them appear not to *prefer* individual family solutions to family problems. At their request, the "individual families" option had originally been included as a response to the "responsibility" question. We were successful in arguing for its removal only after a pretest revealed that it was selected in nearly every case, which rendered it useless in determining differences in group preferences. As a result of their sensitivity on this point, we inserted a disclaimer to the effect that they had based the survey on the *assumption* that the family unit was the primary and preferred institution to handle all of these problems and that the sole purpose of the survey was to determine

FAMILY-RELEVANT CONCERNS OF THE ARIZONA POPULATION

The results of the "concern" questions are given in the lefthand portion of Table 1, which reveals very substantial differences in the level of concern expressed about various issues. The coding scheme used (and described in the footnote to Table 1) means that a lower mean score corresponds to a *greater* level of concern. A broad outline of the findings indicated in this summary table will suffice for the purposes of this paper.

The most conspicuous individual findings were the extremely high concern ratings given to two specific areas: the effects of inflation on families (1), and child abuse and neglect (2).[5] The highest score on the entire survey, that for the effects of inflation on families, is eminently logical, considering that the inflation rate peaked at a record-breaking 18 percent per annum during the week the survey was conducted. Furthermore, it is likely that the high score for inflation is partially attributable to the fact that this is one of the few issues in the entire survey that demonstrably affects nearly everyone. The issue of child abuse and neglect attained virtually the same level of concern. Though child abuse may be less pervasive than inflation, there is evidence that recent publicity about this problem may have produced a substantial increase in the level of public concern about it.

After noting these two overriding concerns, we see that several patterns are apparent in the ratings given the remaining issues. The differences, however, are not quite as striking as those which distinguish the effects of inflation and child abuse and neglect from the other issues. Health-related issues clearly were a high-priority item. Health care for the elderly (3), unsafe or unhealthy working conditions (7), health care for the poor (8), the prevention of health problems (9), and the effects of pollution on health (10) all ranked in the upper quartile of concerns. Other health-related concerns, mental health (16) and prenatal care for pregnant women (20), were in the top half of the list of concerns, but were judged to be of significantly lower concern than were the first group of health-related concerns.

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which sources of outside help would be preferred when intrafamily assistance was inadequate. This disclaimer was prominently featured on all written questionnaires and reports and was read to all telephone respondents who questioned us on the subject. The political instincts of the Governor's Council proved accurate since, in spite of this disclaimer, the charge that they were using the survey to promote an "anti-family" position was still vociferously lodged. That such charges were raised despite these precautions was an indication of just how vituperative the process had become.

[5] Unless otherwise indicated, the numbers in parentheses in this section refer to the relative *ranking* of concerns.

Table 1. Concern Rankings for Selected Family-Related Issues

Random Arizona Sample			*Public Hearings*	
Rank	*Mean Score*[a]	*Question*	*Mean Score*	*Rank*
1	1.242	Effects of inflation on families	1.432	3
2	1.253	Child abuse and neglect	1.221	1
3	1.504	Health care for the elderly	1.560	10
4	1.548	Needs of handicapped persons	1.713	16
5	1.603	Drug abuse	1.499	5
6	1.615	Needs of children whose natural parents cannot care for them	1.530	7
7	1.618	Unsafe or unhealthy working conditions	1.817	24
8	1.680	Health care for the poor	1.656	13
9	1.685	Prevention of health problems	1.581	11
10	1.704	Effects of pollution on health	1.740	18
11	1.725	Alcohol abuse	1.544	8
12	1.751	Discrimination because of race, age, handicap, sex, religion	1.764	20
13	1.779	Education on birth control	1.618	12
14	1.784	Economic aid to needy families with children	1.811	21
15	1.796	Economic aid to retired persons	1.984	28
16	1.824	Mental health	1.687	14
17	1.825	Job opportunities for the unemployed	1.841	25
18	1.856	Needs of families with children in trouble with the law	1.811	22
19	1.869	Spouse abuse	1.555	9
20	1.932	Prenatal care for pregnant women	1.740	17
21	1.935	Opportunities for youth employment	2.013	29
22	1.939	Education on parenthood	1.451	4
23	1.959	Abortion	1.399	2
24	2.012	Job discrimination	2.091	34
25	2.045	Effect of divorce on families	1.704	15
26	2.095	Education on marriage and family life	1.524	6
27	2.115	Child care for working parents	2.068	32
28	2.120	Needs of teenage parents	1.857	26
29	2.203	Education about legal rights and choices for family members	1.945	27
30	2.222	Economc aid to unemployed workers	2.158	36
31	2.224	Needs of single-parent families	1.815	23
32	2.239	Opportunity for ethnic communities to take care of their own problems	2.062	31
33	2.272	Effect of working mothers on families	1.748	19
34	2.276	Education on retirement	2.113	35
35	2.302	Equal rights for women	2.072	33
36	2.366	Needs of ethnic families	2.248	38
37	2.408	Flexible working hours for parents	2.409	40
38	2.448	Education on changing lifestyles such as step-parents, single-parent families	2.021	30
39	2.478	Needs of families living with elderly residents	2.170	37
40	2.514	Special needs of non-English-speaking families	2.338	39
41	2.802	Effects of job transfers on families	2.809	41

[a] 1 = Extremely Concerned; 2 = Moderately Concerned; 3 = Only Slightly Concerned; 4 = Not at All Concerned. Differences in scores must be at least .065 to be statistically significant.

Drug abuse (5) and alcohol abuse (11), related issues with a clear health component, also were judged to be of great concern. Thus, the concern demonstrated about them is consistent with the overall finding that health-related concerns are collectively among the most salient concerns of Arizonans.

Other than the overwhelming concern about inflation already noted, economic issues were clearly of moderate concern. Except for inflation, the highest ranked issue that may be construed to have an economic base was discrimination because of race, age, handicap, sex, or religion (12), which was not even in the top quartile of concerns. In order of relative concern, economic aid to needy families with children (14) was judged of the next greatest economic concern, followed by economic aid to retired persons (15), job opportunities for the unemployed (17), opportunities for youth employment (21), job distrimination (24), child care for working parents (27), and economic aid to unemployed workers (30). With the single exception of inflation, economic issues were judged of lower overall concern than most of the health-related issues dealt with in the survey but were generally of greater concern than most of the other areas covered.

All other sets of issues were ranked substantially lower than either health-related issues or economic-related issues. Among these were issues that some would see as having a "moral" component (ranked 13, 22, 23, 25, 26, 28) and questions dealing with concern about education for various family-related needs (ranked 13, 22, 26, 29, 34, 38). Concerns specifically addressed to various minority groups (ranked 32, 35, 36, 40) and several "lifestyle" issues (ranked 34, 37, 38, 39, 41) were ranked among the lowest of all issues covered in the survey.

INSTITUTIONAL PREFERENCES

The lefthand portion of Table 2 indicates the percentage of the respondents who cited each of the five institutions as the one that should have the major responsibility for assisting families who need outside help averaged across the 41 specific concerns covered. Of course, substantially different results were obtained for individual issue areas. For purposes of this paper, however, some summary findings will prove equally revealing.

Overall, the configuration of institutional responses preferred constitutes a surprising endorsement of state and local government aid for families who need outside help in the areas covered in this survey. Fully 39.27 percent of the responses given represented preferences for state and local government aid in these areas, a figure nearly twice that of the second most preferred institution. Equally impressive was

Table 2. Institutional Preferences for Dealing with Selected Family Concerns

	Mean Percent Response	
Institution	*Random Arizona Sample*[a]	*Public Hearings*[b]
State and local government	39.27%	30.60%
Community organizations	20.27	32.46
Federal government	18.41	16.10
Churches	7.74	24.36
Private businesses	6.62	15.35
Mean number of responses	.9231	1.1887

[a] 2.82% of the respondents insisted on "individual families" after a second probe and 4.86% insisted on a multiple response after a second probe.

[b] Because the public hearings surveys were self-administered, multiple responses were recorded since it was impossible to prevent respondents from giving more than one response to each question.

the fact that state and local governments were given as the most frequently chosen preference for 31 of the 41 questions. State and local governments were chosen as the second most frequent preference in all of the remaining areas. The overall preference for state and local government aid over assistance by any of the other institutions could hardly have been more impressive.

Aid to families who need outside assistance by community organizations (20.27 percent of responses) was approximately as popular as was aid by the federal government (18.41 percent of responses). Both were substantially less popular than aid by state and local governments, and were chosen about half as often as this option. On the other hand, both community organizations and the federal government were substantially more popular than aid by either churches (7.74 percent of responses) or by private businesses (6.62 percent of responses), and were chosen about three times as often as either of these institutions. Churches were the most popular institutional source of aid in only one of the 41 areas and were the second most commonly chosen option only once. Likewise, private businesses were chosen most often only two times out of 41 and were chosen second most often only once.

Data Source II: The Public Hearings Surveys

Self-administered questionnaires were distributed at the six regional hearings for the White House Conference on Families held throughout Arizona in February and March of 1980; 1,389 surveys were returned. Using the results of the random telephone survey as a measure of the composition of the state's population, we may examine the extent to which the characteristics of this sample differ from those of the state's population as a whole with respect to these attributes.

Respondents from the hearings are more likely to be married and living with a spouse (75.0 percent of the hearings sample *vs.* 58.7 percent of the general population sample) and less likely to have any other marital status (separated, widowed, divorced, or never married), especially never married (12.3 percent *vs.* 22.4 percent). Persons responding at the hearings are also more likely to either have or live with children (79.6 percent *vs.* 65.5 percent). There are more whites (91.3 percent *vs.* 86.5 percent) and fewer Hispanics (3.7 percent *vs.* 8.3 percent) and blacks (0.7 percent *vs.* 2.2 percent) in the hearings sample than in the state's population. Respondents from the public hearings also have higher incomes than the statewide sample (with a greater proportion of persons in each category over $20,000 and a smaller proportion in each category under $20,000). Respondents in the hearings sample are substantially more likely to be Mormon (26.6 percent *vs.* 5.0 percent) and less likely to be either Catholic (15.7 percent *vs.* 24.6 percent) or Protestant (35.0 percent *vs.* 53.0 percent) than the general state population. The hearings sample also contained an even greater proportion of females (72.4 percent *vs.* 58.0 percent) than the general population state survey. Pima county residents (Tucson) were overrepresented and Maricopa county residents (Phoenix) were underrepresented at the hearings. The only demographic variable for which there was no appreciable difference between the samples was age; in both samples the mean age was approximately 40 years.

RELATIVE CONCERNS OF HEARINGS PARTICIPANTS

The mean concern ratings and relative rankings of the 41 issues covered in the survey distributed at the public hearings are given in the righthand portion of Table 1. There are a number of differences in the relative priority of various concerns by the public hearings and the general population samples. The two conspicuously paramount concerns in the random Arizona sample, child abuse and neglect and the effects of inflation on families, are both still substantial concerns of the public hearings sample, but their prominence is no longer conspicuous. Issues that have a "moral" component (abortion, education on marriage and family life, education on parenthood, and the effects of divorce on families) experienced the most dramatic increase in salience as did spouse abuse. Issues that impact on the traditional two-parent family (the effects of working mothers on families, needs of single-parent families, and education on changing lifestyles such as stepparents and single-parent families) also increased in salience, although less dramatically.

Many of the differences in priorities observed between the random statewide sample and the public hearings sample appear to be the result of demographic differences between the two samples. In the differences just noted, for example, the increased salience of "moral" issues might appear to be attributable to the overrepresentation of Mormons, who tend to be fundamentalist in outlook. Similarly, the overrepresentation of married persons and persons with children among hearings participants might appear to be responsible for the greater level of concern given by these respondents to issues that impact the traditional two-parent family. To assess whether these and other differences were due to the demographic composition of the hearings sample, we undertook nearly 200 ANOVAs. To save space, we will summarize rather than present detailed findings of these analyses.

While there was a tendency for Mormons in the statewide RDD survey to be more concerned than other religious groups about the "moral" issues covered, differences in concern among hearings participants were nonsignificant. Furthermore, for nearly all groups, there was a greater level of concern about "moral" issues expressed by hearings participants than by persons of the same religion in the random statewide sample. From these facts, we conclude that divergent findings in the two samples were due to selection *within* religious groups rather than merely the overrepresentation of some religious groups at the hearings. Findings for concerns related to the traditional two-parent family were similar. Differences between the hearings and random statewide samples for these variables were not attributable to the greater prevalence of married persons and those with children in the hearings sample. (Other comparisons were also made with similar results).[6]

The primary relative and absolute drop in concerns among the public hearings sample was felt in issues related to economics and health. Of the 13 concerns that dropped at least five ranks relative to their rank by the general population sample, seven concerned economic issues and four were health-related. Because these were the two most highly rated general areas of concern in the random statewide survey, these findings are indicative of a substantial departure from the priorities of the general population.

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[6] This self-selection within demographic groups may, in turn, have been "explained" by political partisanship, ideology, or any of several surrogate variables. Unfortunately, in order to include *any* demographic data in the survey, we had to engage in extensive lobbying of the Governor's Council, whose extreme sensitivity to political repercussions we have already noted. Our "negotiated settlement" was a compromise that permitted us to include a few demographic items, and partisanship was not among these.

With respect to certain health-related issues, there were some differences in the effects of various demographic variables. While there were no significant differences in concern about health care for the elderly between different ethnic groups in the random statewide sample, whites at the hearings were significantly less concerned than others, accounting for the lower overall concern of hearings participants with this issue. Hearings participants within each income category were slightly less concerned about this issue than their counterparts in the statewide sample. Catholics and Jews at the hearings were more concerned about health care for the elderly than their religious counterparts in the random statewide sample, while Mormons and Protestants at the hearings were less concerned about this issue than their coreligionists.

These observations suggest a pattern that was evident for many of the issues addressed in the survey. If we define a social conservative as one who is less concerned with social welfare issues and more concerned with "moral" issues and a social liberal as one whose concerns run in the other direction in each of these issue areas, then we can observe that social conservatives were disproportionately represented among Mormons at the hearings and that social liberals were disproportionately represented among racial minorities, Jews, and (sometimes) Catholics at the hearings. Note that we are not simply asserting, for example, that Mormons tend to be social conservatives (although this is also the case), but also that those Mormons who attended the hearings were even more socially conservative than are Mormons in the general Arizona population. Precisely the reverse was true for racial minorities, Jews, and Catholics with respect to most of the concerns covered in the survey.

With some distinctions, the relationships for other health-related concerns were generally similar to those for health care for the elderly. The lower level of concern about unsafe or unhealthy working conditions, health care for the poor, and the effects of pollution on health expressed at the hearings could not be fully explained by the ethnic, economic, or religious composition of the hearings. Rather, overall differences between hearings participants and the general population within demographic groups appear to account for differences in the concerns of the two samples. On the other hand, for five of these nine health-related concerns, the demographic composition of the hearings could account for at least *part* of the difference between the random statewide findings and those for the hearings sample.

An examination of the seven economic concerns that dropped at least five ranks by ethnicity, religion, and income level indicated that the diminished concern expressed at the hearings might have been due to the demographic composition of the hearings. Minorities, low income persons, and Catholics in the random statewide survey all tended to express greater than average concern about these economic issues, and each of these groups was underrepresented in the public hearings sample. We therefore examined the relationships between demographic attributes and economic concerns within the two samples. Our analyses made it evident that while some of the differences could be substantially explained by the decreased representation of these groups at the hearings, for other issues, the self-selection of individuals *within* ethnic, religious, or economic groups was more significant in accounting for the lower relative concern for economic issues expressed at the hearings. Overall, the crude demographic discrepancies between the hearings and general population samples were less important in explaining divergent priorities than were self-selection mechanisms within various demographic groups. While self-selection mechanisms for hearings attendance were somewhat demographically biased, they were even more strongly biased along the liberal/conservative dimension.

The other notable aspect of the findings for the "concern" question is the wide range of groups whose needs were rated as substantially less salient to the public hearings sample than to the general population sample. These included retired persons, the handicapped, the unemployed, youths, the elderly, and needy families with children. This hints at the somewhat startling finding that those persons for whom the "moral" issues take precedence (who were overrepresented at the public hearings) are significantly less willing to support social services not only for the needy groups that have often been characterized as relatively less "deserving" (e.g., unwed welfare mothers) but also for groups that have traditionally been more favorably viewed (e.g., handicapped and elderly persons). Limited analyses of differences in concerns between groups within each sample are consistent with this observation, suggesting an important area worthy of further research with significant policy implications.

INSTITUTIONAL PREFERENCES

The righthand column of Table 2 indicates the extent to which the institutional preferences of the public hearings sample diverge from the preferences of the general population sample. Because the self-administered surveys yielded a much higher proportion of multiple responses to this question, they were coded so as to permit recording these multiple responses. The total number of responses could therefore exceed 100 percent. These differences must be taken into account when comparing these results with those of the random telephone survey.

Despite this caveat, differences in the institutional preferences of the two samples are still striking. Respondents from the public hearings are more inclined to prefer relying on community organizations and churches and less likely to prefer government aid (whether at the state, local, or federal level) than are respondents from the random statewide sample. State and local government aid, however, still ranks near the top of the institutional preferences of the public hearings sample, although for this sample, state and local governments compete with community organizations for designation as the "most preferred" source of institutional assistance for families in need. Private businesses, on the other hand, were chosen least often by both samples. The movement towards more private and voluntary sources of aid within the public hearings sample is consistent with the lower priority given to economic and health-related concerns and the greater emphasis on "moral issues within that sample.

Conclusions

One risk of the public hearings process is that the misinformed may come to equate the views expressed at such meetings with the sentiments of the general public. Under circumstances like those examined in this paper, public hearings are, at best, a means of gauging the opinions of a particularly well-mobilized segment of the community. While the capacity of public hearings to be unrepresentative of the overall population should not surprise any astute observer of the process, hard empirical data confirming the nature of the discrepency has been scant. By comparing data on certain aspects of Arizona citizens' preferences for governmental aid to families with comparable data on preferences of participants in a series of hearings held throughout the state to provide public input on those priorities, we have an initial indication of some possible sources of selectivity.

In this particular case, public hearings participants were demonstrably more concerned about certain "moral" issues and less likely to favor aid to families by governmental entities than was the general public. Collectively, these are orientations characteristic of persons who are religious fundamentalists and political conservatives. The overrepresentations of whites, persons with high incomes, and Mormons at the hearings are consistent with these observations. But these overrepresentations did not, by themselves, account for the attitudinal differences we observed at the hearings. While the extent to which demographic composition "explained" differences between the statewide and hearings samples varied by issue area, self-selection within groups was generally far more important than demographic overrepresentation of certain religious, ethnic, and economic groups in explaining the differences in the preferences of the two samples.

It is equally intriguing that, in the face of the "capture" of these hearings by those disposed to conservatism and fundamentalism, there was evidence that the selectivity worked in precisely the opposite direction within certain subgroups. This fact was obscured in the overall findings because these groups comprised such a small proportion of the hearings sample. The observation, however, does indicate that competing tendencies may be obscured in overall findings. It also suggests that different groups may be equally successful in "capturing" other public hearings, depending on their ability to mobilize their supporters. Thus, there need be nothing generalizable about the configuration of groups that happened to be overrepresented at these particular hearings. Some of these overrepresented groups (persons of higher socioeconomic status, whites, and women), however, will generally be easier to mobilize in nearly any context.

The "unrepresentativeness" of a particular hearing need not be taken as an indictment of the hearings process—only of claims that assert a "public mandate" from such hearings. Public hearings need not be representative in order to serve a useful function. Hearings may serve a "safety valve" function, allowing the expression of opinions by groups that feel left out of the system, thereby encouraging further participation rather than alienation. They may also serve as forums for the free exchange of viewpoints. Even when they are used more to exert influence than to exchange ideas, the danger is not in the assertion of legitimacy based on the consensus of a public hearing, but in the naive acceptance of claims that they represent a public mandate from a representative body.

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An Elite View of the Polls

JEFFREY L. BRUDNEY

RECENT literature examining public opinion polling in election campaigns attests to the increasing importance of this tool to political elites (e.g., Germond and Witcover, 1981; Ladd and Ferree, 1981; Caddell and Wirthlin, 1981; Cantril, 1980; Wheeler, 1976; Field, 1971). The use of polls for a variety of purposes (allocating candidate time and resources, attracting financial contributors and campaign staff, etc.) has been documented, largely on the basis of the impressions of pollsters, media representatives, and candidates themselves of the critical factors in various electoral contests. Despite this research attention, the views of election polls held by political elites have not been the subject of systematic empirical inquiry. The present study explores this topic through a survey of delegates elected to the 1980 state conventions of the Democratic and Republican parties of Oklahoma, convened during the spring to select delegates to the national party conventions. The paper investigates the attitudes of the delegates in two areas: trust in the results of election polls and perception of their possible impact on voters.

Abstract This research note examines the views of election polls of a sample of political activists: delegates elected to the 1980 state conventions of the Oklahoma Democratic and Republican parties. Findings show that although just 13 percent of the delegates overall place high levels of trust in the results of major national polls, nearly a majority (45 percent) feel that poll results affect voter behavior. Both trust in election polls and perception of their impact on voters are found to increase with involvement or interest in electoral politics.

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Hypotheses

Two hypotheses guide this research. By virtue of their status as delegates, all individuals in the sample have some claim to the designation "political elite." Nevertheless, the level of interest, activism, and experience in politics among delegates varies tremendously. For many delegates, attendance at the 1980 state party convention marks their first active participation in politics, aside from voting, whereas other delegates have been involved in several election campaigns and/or have long enjoyed positions of party leadership or elected office. It is assumed that the greater the activism and experience in politics, the greater the exposure to and appreciation of the uses and advantages of election polls in political campaigns. Accordingly, the first hypothesis is that confidence in poll results and perception of their impact on voters increase with the level of political activism.

Delegates also differ with respect to the primary focus of their political activity at the local, state, or national level. Most polling of political opinion is concentrated on the national level, with decreasing attention devoted successively to the state and local levels. Greater exposure to election polls may differentiate attitudes toward them. Thus, the second hypothesis is that elite trust in polls and belief in their effects varies with the focus of political involvement—delegates involved primarily in national politics will exhibit greatest trust and perception of impact, followed by those active in state politics, and finally by those working at the local level.

Data

Data to evaluate these hypotheses were obtained from delegates elected to the 1980 state conventions of the Democratic and Republican parties of Oklahoma. At the conventions, each delegate received a self-administered questionnaire; 69 percent completed the questionnaire, with virtually no difference in participation by party, yielding a sample of 1,853 delegates.

The questionnaire included two items pertaining to polls of political opinion. The first asked delegates to indicate on a 5-point scale the extent to which "you trust results from major national polls regarding voters' preferences." This item is used as an indicator of confidence in the polls.

The second question, again recorded on a 5-point scale, asked: "To what extent do you feel the results from major national polls regarding voters' preferences affect the way people vote?" The question was presented to the delegates for two reasons. First, although the operation of neither "bandwagon" nor "underdog" effects among the electorate has been confirmed empirically (Meadow, 1980; Gollin, 1980; Lipset, 1976), most researchers allude to the tentativeness of this finding. For example, Edward Tufte notes that in regard to the possible influence of election polls, "The absence of evidence is not necessarily the evidence of absence" (quoted in Dionne, 1980:26). The responses of the delegates provide a new perspective on the issue of poll effects. Second, the item serves as an indicator of elite perceptions of the influence of election polls on voters, a belief that may be actualized in behavior. Party activists who hold the view that polls affect voter behavior are more likely to rally around an appealing candidate as judged by the polls and abandon a flagging one—an election phenomenon described extensively by Wheeler (1976).

Findings

According to the first hypothesis, trust in the results of election polls and perception of their impact on voters are expected to increase with experience and activism in politics. Table 1 shows that 13 percent of the delegates report trust in the results from major national polls to a "great" or "very great" extent; a much more substantial proportion—45 percent—believe that these results affect the way people vote. Contrary to hypothesis, general measures of political activity such as length of participation in party politics,

Table 1. Trust in Election Polls and Perception of Their Impact on Voters

To what extent do you trust results from major national polls regarding voters' preferences?	
Very great extent	3%
Great extent	10
Moderate extent	43
Some extent	30
No extent	14
	100[a]
	(1762)
To what extent do you feel the results from major national polls regarding voters' preferences affect the way people vote?	
Very great extent	11%
Great extent	34
Moderate extent	34
Some extent	17
No extent	4
	100[b]
	(1752)

[a] Five percent of cases ($N = 91$) "not ascertained."

[b] Six percent of cases ($N = 101$) "not ascertained."

election as a delegate to a previous state party convention, and level of involvement in recent state and national election campaigns are not associated with increases in these figures. The gamma statistics assessing the magnitude of relationship between these variables and the poll items in contingency table analysis are negligible, ranging from .01 to .10.

These general measures of political activity, however, do not take into account the type of job or service performed by party delegates. Delegates may hold various positions of formal leadership both within the party and in public office. Table 2 examines the relationship between the type of political position held presently or in the past by delegates and their confidence in poll results and view of the effect of polls on voters. Like the general measures of political activism, attaining party office[1] does not increase trust in election polls or perception of their impact on voters. Fifteen percent of state central committee members, the group of party officeholders manifesting highest confidence, indicate "great" or "very great" trust in poll results, surpassing by just 2 percent the rate for delegates not holding office. Similarly, the perception of poll impact among members of congressional district committees (49 percent indicating "great" or "very great" extent) is only marginally greater than that demonstrated by nonofficeholders (45 percent).

In contrast, holding public office is associated with these attitudes toward election polls. Fifteen percent of delegates appointed to office, 19 percent of those elected to local office, and 24 percent of those elected to state or national office report trust in polls to a "great" or "very great" extent, compared to 13 percent of delegates not holding office. The trend is quite similar with respect to belief in the effect of polls on the electorate. Moreover, as expected by the second hypothesis, confidence in the results of polls and perception of their impact on voters increase gradually with the scope of elective office from the local to the state and national level. However, this relationship is not replicated among party officeholders.

The findings displayed in Table 2 suggest that vulnerability to the

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[1] In the sample of delegates, there is a significant overlap between holding party and public office. Fully 83 percent of public officeholders ($N = 251$) have held party office, and 19 percent of party officeholders ($N = 1{,}113$) have attained public office. In order to minimize possible contamination between the effects of the two types of office, in Table 2 party officeholders have been purged of delegates who have also attained public office. However, because elimination of party officeholders from the ranks of public officials would have left too few cases for analysis ($N = 42$), in Table 2 public officeholders include delegates who may also have experience in party office (i.e., 83 percent). Delegates who have served in both types of office closely resemble those who have only held public office.

Table 2. Trust in Election Polls and Perception of Their Impact on Voters by Position

		Party Office			Public Office			
Extent	*No Party or Pub. Off.*	*Local Party Comm.*	*Cong. Dist. Comm.*	*State Cen. Comm.*	*Apptd. Off.*	*Elec. Loc. Off.*	*Elec. State/ Natl. Off.*	*Campn. Staff for Cand.*
Trust results[a]								
Very great	2%	3%	3%	4%	6%	4%	7%	7%
Great	11	10	8	11	9	15	17	25
Moderate	43	42	41	40	48	44	42	34
Some	29	30	33	30	29	27	22	26
None	15	15	15	15	8	10	12	8
	100	100	100	100	100	100	100	100
	(639)	(678)	(113)	(101)	(117)	(131)	(41)	(73)
Polls affect vote[a]								
Very great	11%	9%	10%	6%	15%	12%	13%	11%
Great	34	35	39	41	36	38	43	42
Moderate	32	36	32	33	34	34	25	37
Some	17	16	16	18	14	13	17	7
None	6	4	3	2	1	3	2	3
	100	100	100	100	100	100	100	100
	(634)	(677)	(114)	(100)	(117)	(130)	(40)	(73)

[a] For complete wording of items, see Table 1.

will of the electorate heightens belief in election polls and their possible consequences. This effect is found not only among delegates elected to office but also those serving on the campaign staff of a candidate, a position in which the proximity to and ramifications of electoral politics are equally immediate. Additional data provide support for this conclusion. Delegates were asked how important a factor in their decision to become involved in the 1980 presidential campaign was their desire to help their own political career. In addition, they indicated the extent to which they were interested in becoming candidates for public office. These variables are cross-tabulated with the election poll items in Table 3.

Table 3 shows that trust in the results of election polls to a "great" or "very great" extent rises steadily from 10 percent among delegates who did not consider aiding their own political career at all important in their decision to become involved in 1980, to 26 percent for those who rated this factor very important in their participation. The relationship between interest in running for public office and confidence in election polls is quite similar. The belief among convention delegates that polls affect the way people vote also increases with interest in a political career. For example, a 16-percentage-point difference separates delegates most interested in attaining public office from those least interested. Trust in the results of election polls and per-

Table 3. Trust in Election Polls and Perception of Their Impact on Voters by Political Aspirations

	Help Political Career				*Interested in Public Office*		
Extent	*Very Imp.*	*Somewhat Imp.*	*Not Very Imp.*	*Not at All Imp.*	*Very Much*	*Sonewhat*	*Not at All/ Not Sure*
Trust results[a]							
Very great	9%	5%	1%	1%	7%	3%	2%
Great	17	20	13	9	19	11	9
Moderate	44	45	50	42	44	50	41
Some	24	20	24	32	20	27	31
None	6	10	12	16	10	9	17
	100	100	100	100	100	100	100
	(93)	(157)	(228)	(1081)	(147)	(363)	(1229)
		gamma = .26				gamma = .23	
Polls affect vote[a]							
Very great	19%	15%	12%	9%	21%	11%	9%
Great	39	35	37	35	37	38	33
Moderate	25	35	36	33	31	34	35
Some	9	10	12	19	8	14	18
None	8	5	3	4	3	3	5
	100	100	100	100	100	100	100
	(90)	(156)	(227)	(1077)	(146)	(362)	(1221)
		gamma = .14				gamma = .17	

[a] For complete wording of items, see Table 1.

ception of their impact on the electorate are highest among delegates with electoral aspirations.

In summary, two findings of this study seem especially important. First, among the sample of Oklahoma convention delegates, the rate of significant trust in election poll results seems surprising low, ranging between 11 and 32 percent for selected groups, and only 13 percent overall. Nevertheless, nearly a majority of delegates (45 percent) believe that polls affect voter behavior, and the percentage rises among those involved or interested in electoral politics. While the connection between attitude and behavior must remain a matter of conjecture, the generality of this view across delegates may help to account for the migration of funds and other support from candidates languishing in the polls to those blessed with favorable ratings in them (cf. Wheeler, 1976). Perhaps, then, as others have noted, the more serious types of bandwagon effects are not to be found among voters, but among political elites.

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Blacks, Whites, and Attitudes Toward Abortion

MICHAEL W. COMBS AND SUSAN WELCH

In THIS paper, we examine patterns of black and white support for abortion from 1972 to 1980 in order to: (1) investigate the significance of race in attitudes toward abortion; (2) analyze the extent to which other factors such as religious practice (religiosity) and demographic characteristics affect these racial differences; and (3) determine what changes, if any, occurred in the salience of race on abortion attitudes during this nine-year period.

While earlier studies revealed liberalizing trends in abortion attitudes among both blacks and whites, blacks tended to be less permissive about abortion than whites (see Harter and Beasley, 1967; and Lipson and Wolman, 1972). Four factors may account for this. First, previous studies of abortion attitudes have noted that more formal education and higher occupation and income status correlate with support for abortion (Blake, 1971; Tedrow and Mahoney, 1978:185; Skerry, 1978; Singh and Leahy, 1978; and Ebaugh and Haney, 1980:496). Since blacks have, on the average, less income, less formal

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Abstract This article examines patterns of black and white support for abortion from 1972 to 1980. The findings reveal that black-white differences are present on the abortion issue. Many of the differences are due to the different demographic characteristics of blacks and whites and the greater degree of religiosity of blacks.

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education, and lower occupational status than whites, some of the racial differences in abortion attitudes may be an artifact of class differences. If this is so, increasing social and economic status among blacks may contribute to an increase in support for abortion among blacks (see Dahl, 1960; Glazer and Moynihan, 1963; and Wilson, 1978).

Second, religious practices and belief may help to explain black-white differences, because attitudes toward abortion have been found to correlate with religion and church attendance (Singh and Leahy, 1978; Granberg, 1978:420; Tedrow and Mahoney, 1979; and see Welch, 1975). Catholics and frequent church attenders are more opposed to abortion than non-Catholics or infrequent churchgoers (Ebaugh and Haney, 1980). In addition, in the black community, the church may "represent a strong tie to the traditional (black) culture" (Aberbach and Walker, 1970:377). If blacks attend church more often and are more committed to traditional religious practice, this may also be an explanation of black-white differences.

A third force increasing racial differences in abortion attitudes may be the black political movement that surfaced in the 1960s. This movement emphasized that political strength is in numbers and abortion is a "white man's plot" to commit genocide in the black community (see Sloane and Horwitz, 1973). Race consciousness that accompanied the black political movement of the 1960s may support high fertility (Roper et al., 1978:445), and, thus, opposition to abortion. The women's liberation movement, however, has affected the attitudes of black women (Fulenwider, 1981:26), cross-pressuring them with the demands of controlling their own destinies as women as well as black people. In the 1960s, women were less permissive toward abortion than men (Black, 1971); however, by 1978 the differences were negligible (Ebaugh and Haney, 1980).

The fourth source of black-white differences in abortion attitudes may be the influence on black attitudes of southern and rural culture. Blacks reared in or socialized in the South are more traditional than other blacks in their views on many social and political issues (see Aberbach and Walker, 1970a, 1970b) and perhaps in matters of personal morality as well (Granberg, 1978:424). The influence of southern and rural culture may also extend to the abortion issue (see Black, 1971:541 and Granberg, 1978:418). The rate of disapproval of abortion among southerners was greater than that of Catholics, who are the most conservative religious group on abortion. On the other hand, the increasing urbanization of blacks may tend to promote convergence of black-white differences.

Our working hypotheses are: (1) Although racial differences toward abortion have diminished in the 1970s, some differences still exist. (2) Differing demographic characteristics of blacks and whites will account for some of the racial differences in these attitudes. (3) Differing religious practices will also account for some of the racial differences in these attitudes.

Methods and Data

To assess racial differences in attitudes toward abortion, we used the General Social Surveys (GSS) conducted by the National Opinion Research Center of the University of Chicago from 1972 to 1980. These surveys, done annually with the exception of 1979, use national samples of approximately 1,500 respondents each year. A modified probability sampling design was used in 1972 through 1974, full probability sampling in 1977–1980, and a combination of the two in 1975 and 1976. Six standard questions tapping abortion attitudes were asked in each of these surveys. These questions measured support for the availability of abortion under circumstances ranging from damage to the health of the mother through a preference by the woman to have no more children. The exact wording of the questions are:

> Please tell me whether or not *you* think it should be possible for a pregnant woman to obtain a *legal* abortion if . . .
> A. If there is a strong chance of serious defect in the baby?
> B. If the woman's own health is seriously endangered by the pregnancy?
> C. If she became pregnant as a result of rape?
> D. If she is married and doesn't want any more children?
> E. If the family has a very low income and cannot afford any more children?
> F. If she is not married and does not want to marry the man?[1]

From these six items an additive scale was created, with values ranging from 0, when abortion was opposed in every case, to 6, where approval was given for abortion in each case. This scale was used as a dependent variable in the analyses.[2]

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[1] The items were asked in the following order: A, D, B, E, C, F.

[2] A Guttman scaling procedure on the six items was also done for each race each year. In general, the six items are more scalable for whites than for blacks. Whites have "errors" of only 5–6 percent, while the "error rate" among blacks ranges from 8 percent in 1976 to 11 percent in 1975. This means that there is slightly less homogeneity among the black than the white community in the way individuals view the relative acceptability of the six circumstances of abortion. Because of the higher "error rate" among blacks, making the scales in some cases above an acceptable error rate for a Guttman scale, we used only the simple additive scale in our analysis.

After an examination of the abortion items on a year-by-year basis, the surveys were grouped into three clusters: 1972–1974, 1975–1977, and 1978–1980. This allows us to eliminate some sources of error due to the small number of blacks in each sample. And because changes from year to year in these attitudes are not dramatic, no striking trends are masked by the merger of more than one year.[3] We examined simple differences between blacks and whites, and then computed differences between the races after controls for demographic factors. Finally, differences were computed after adding controls for religious practice to the demographic controls. The difference still remaining between blacks and whites was the difference we could not account for by other means. Following that analysis, we turned to an examination of differences in abortion attitudes among blacks themselves. We sought to determine if the same factors explaining attitudes in the whole sample also did so for blacks alone.

Controls were included for the following demographic and attitudinal factors: family income, years of education, region of residence (South=1, North=0), region of residence at age 16 (same codes), size of place of residence (central city=1, suburb=2, other=3), rural residence at age 16 (rural=1, other=0), and sex (females=1, males=0). Family background as evidenced through mother's and father's education was also examined, but eliminated because of problems of missing data. Religious practice controls included the frequency of church attendance (ranging from never through less than once per year through more than once per week) and denominational membership.[4] A final measure of religious attachment is the respondent's self-identification as a strong (1), not very strong (3), or somewhat strong (2) member of his or her religion or denomination. Race was a simple dichotomy, with those of races other than black or white excluded (black=1, white=0).

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[3] A survey-by-survey analysis shows a lower level of support than is reflected here for the "hard items" by blacks in 1972. Either this was a statistical anomaly or the Roe decision in 1973 boosted black support.

[4] Dummy variables were created for Roman Catholics and members of fundamentalist groups. The latter measure is a weak measure, however, because of the way NORC codes religion. For example, all Baptists are given a single code although white southern Baptists could be considered fundamentalists while other Baptist groups would probably not be (see Glock and Stark, 1965). Likewise, Missouri Synod Lutherans are more fundamentalist than other Lutheran denominations, yet all are coded together. Our fundamentalist dummy variable included Baptists plus members of numerous sects considered to be fundamentalist. These groups were some of the numerous sects coded separately by NORC. Examples are Holy Rollers, Church of God, and Pentecostals.

Findings

DIFFERENCES BETWEEN BLACKS AND WHITES

Table 1 displays the proportions of each race favoring abortion. The overall impression from Table 1 is that there has been a great deal of stability in public attitudes toward abortion. This is especially true for the health, rape, and birth defect items, where little change is evident in either race, except for increased black support for abortion when the mother's health is threatened.

For the other items, support by blacks appears to have increased during this time, while support by whites dropped slightly. However, the amount of convergence was not statistically significant,[5] according to methods described by Taylor (1980) for testing this significance. While blacks remain significantly less likely to favor abortion in all six instances, they are, by 1978–80, almost equally unlikely to oppose abortion in all cases (9 percent for whites, 12 percent for blacks). This is the only black-white difference in Table 1 that is not statistically significant. The mean number of abortion items supported by blacks rose slowly from 3.1 in 1972–74 to 3.3 in 1978–80, while white support dropped very slightly from 4.0 to 3.9 in 1978–80 (though returning to 4.0 in 1980).

EXPLAINING RACIAL DIFFERENCES IN ABORTION ATTITUDES

To determine if the racial differences we have found are explainable by other factors, we examine the effect of race on the 7-point (0–6) abortion scale under three different conditions (Table 2). In row 1 we see that the simple differences between the two races are in all cases significant, although they diminish over time.[6] In row 2 we show the differences between blacks and whites after demographic factors are controlled. In all cases, the differences are still significant but diminished by about one-third over the uncontrolled means. And by 1975, the effects of race after the demographic controls are quite weak even though statistically significant, with betas of only .06.

When we add the religious practice variables to the equations (row 3), black-white differences are reduced to a small and statistically insignificant level in 1975–1977 and to a quite small level in 1978–1980, with a beta value of only .04. The mean difference between

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[5] Taylor (1980) describes a simple method for testing whether changes evidenced between groups over time are simply random fluctuations or whether they are "true" differences. Briefly, the method involves a chi-square goodness of fit test whereby "expected" group differences are compared with actual ones.

[6] The differences here should parallel those in Table 1. Missing data on the other variables and hence removal of cases from the analysis accounts for the slight variation.

Table 1. Change in Attitudes Toward Abortion by Race, 1972–1980

	(% Favoring)					
	1972–1974		*1975–1977*		*1978–1980*	
	Black	*White*	*Black*	*White*	*Black*	*White*
Woman's health	81	92	82	92	85	91
Rape	68	85	70	85	70	85
Birth defect	67	85	69	86	68	84
Can't afford	41	54	46	54	44	50
Single	33	50	37	51	35	46
Wants no more	36	46	41	47	40	44
% favors all 6	20	37	24	38	23	35
% opposes all 6	16	7	13	7	12	9*
Mean scale score	3.06	3.96	3.29	4.00	3.30	3.87

* Black-white difference not significant at .05

blacks and whites in 1978–1980 (.26), though not as small as in 1975–1977 (.16), was smaller than in 1972–1974 (.49) and smaller than the difference without the religious controls. This indicates that a good deal of racial differences in attitudes toward abortion can be explained by the greater religious participation of blacks: blacks are more likely than whites to attend church regularly and to perceive themselves as having a strong commitment to their religion or denomination. They are also more likely to be classified as fundamentalist, although less likely to be Roman Catholics. Since religious attendance, Roman Catholicism, and fundamentalism each have a significant negative impact on abortion attitudes, controlling for religious practice does reduce black-white differences in abortion attitudes.[7]

Most of the demographic and religious controls did have statistically significant relationships, in the predicted direction, with abortion. However, because of the very large pooled sample sizes (2,700 to 4,100), statistical significance can be achieved with quite weak relationships. Thus, we present in Table 2 only those demographic and religious variables that achieved a beta value of at least .10 with abortion. Interestingly, the same variables achieve this level in each equation. Of the demographic characteristics, only education achieved a beta of at least .10, and it did so each time. Each year of education increases support for abortion from .09 to .14 points on the

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[7] The relationship of these religious practices to abortion attitudes should not be overstated, however. The 1978–1980 correlation of abortion attitudes with membership in the Catholic church was only −.13, with fundamentalism −.13, but with church attendance and strength of denominational or religious preference a more robust −.35 and .34, respectively.

Table 2. Impact of Race on Abortion Attitudes[a]

	1972–1974	*1975–1977*	*1978–1980*
1. Race alone	−.95(−.16)	−.66(−.10)	−.59(−.09)
2. Race, controls for demographic variables[b]	−.61(−.10)	−.39(−.06)	−.39(−.06)
3. Race, controls for all other variables[c]	−.49(−.08)	−.16(−.02)*	−.26(−.04)
4. Other variables with betas ≥ .10			
Education	.14 (.22)	.11 (.17)	.09 (.16)
Roman Catholic	−.62(−.14)	−.60(−.13)	−.66(−.14)
Attendance	−.21(−.28)	−.19(−.25)	−.16(−.22)
Strength of affiliation		.22 (.11)	.29 (.15)
R^2	.24	.23	.22
(*N*)	(4,170)	(4,073)	(2,718)

* Not significant at .05.

[a] Six-point abortion scale where 6 = support for each type of abortion; 0 = support for none. Figures are unstandardized regression coefficients, followed by standardized coefficients in parentheses. Unstandardized regression coefficients for race measure the difference between whites and blacks on the abortion scale—thus, an unstandardized regression coefficient of −.61 means that blacks score .61 points lower than whites after adjusting for the controls listed in footnote b.

[b] Controls for income, education, age, region, size of place of residence, sex, region, and rural-urban residence at age 16.

[c] Controls listed in footnote b plus dummy variables for Roman Catholic and fundamentalist religious affiliation, and controls for frequency of church attendance and self-perceived denominational commitment (the latter item was not asked in 1972 and 1973 so it was excluded from the 1972–1974 regression.

scale, representing betas of .16 to .22. The impact of education appears to be decreasing very slightly. Though southern and rural childhood residence, low income, and current rural residence were each related to abortion attitudes in the expected direction (southerners, rural residents, and those with low income were less supportive), none of the relationships exceeded .10 in any of the regressions. Women were more supportive of abortion (at a significant level) than men, controlling for the other factors, while age was unrelated in two of the three regressions.[8]

The religious variables have a consistently stronger impact than the socioeconomic ones, with Roman Catholicism and church attendance related to abortion attitudes with betas greater than .10 in all three regressions. Strength of attachment to denomination or religion was also related at that level in the two regressions in which it was entered. Church attendance has the strongest relationship with abortion of any of the variables (betas ranging from .22 to .16). The influence of Roman Catholic affiliation has stayed about the same. Fundamentalists are significantly less likely to favor abortion, but the strength of the relationship is quite weak (beta = .05 in 1978–80), perhaps in part illustrating the coding difficulties with that variable.

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[8] The opposite than predicted effect for gender indicates that the lack of significant bivariate effects of gender on abortion attitudes is due to the surpressor effect of religious practice; women are more likely than men to attend church frequently and have a high religious commitment. Once that is controlled, women are more supportive of abortion. Or put another way, at each level of religious commitment women are more likely than men to favor abortion.

Thus, our analysis of the demographic and religious variables largely confirms several of our explanations for black-white differences in abortion attitudes: blacks have less education and income, and are more likely to be rooted or have current residences in the South, and are more likely to have religious practices associated with anti-abortion attitudes. Beyond this, however, race still has a small, but significant, effect.

DIFFERENCES AMONG BLACKS

While we have inferred that the higher level of religious commitment and lower education and income levels among blacks have contributed to their less favorable attitudes toward abortion, the preceding analysis has not told us anything specific about attitudinal differences *among* blacks. Here we will briefly examine differences within the black community.

Table 3 indicates that we can explain about as much variation within the black community as in our sample as a whole—from 21 to 23 percent. Generally, the same patterns emerge as for the sample as a whole. Blacks with more formal education and income favor abortion more than other blacks, but the differences are not always significant, partly because of the small sample size. The size of the estimated effects are also smaller than in the sample as a whole. Church attendance is strongly and consistently related to abortion attitudes across the three pooled analyses.[9] Urban residence, northern residence, and being female are positively related to support for abortion, though each relationship is statistically significant in only one regression. The major deviation from the results reported in the last section is the insignificant effect of Roman Catholicism, explainable by the few black Roman Catholics in the survey. Whether these black Catholics are reflective of all black Catholics is impossible to say, owing to their small number. With these exceptions, however, patterns in attitudes toward abortion in the black community mirror those in the larger society, and thus seem to be shaped by the same forces as white attitudes.

[9] Strength of denominational or religious preference was omitted from the analysis because of multicollinearity with church attendance.

Table 3. Impact on Black Attitudes Toward Abortion of Demographic and Religious Factors[a]

	1972–1974	*1975–1977*	*1978–1980*
Income	.05 (.08)	.08 (.12)*	.07 (.11)
Education	.17 (.31)*	.06 (.11)	.08 (.13)
Sex	.32 (.08)	.12 (.03)	.48 (.12)
Age	.00 (.03)	−.01(−.08)	−.00(−.04)
Size of residence	−.04(−.02)	−.24(−.09)	−.03(−.01)
Region	−.42(−.10)	−.04(−.01)	−.82(−.20)*
Rural residence—Age 16	−.17(−.04)	−.61(−.14)*	−.12(−.03)
Southern residence—Age 16	.34 (.07)	−.05(−.01)	−.34(−.08)
Roman Catholic	−.02(−.00)	−.03(−.00)	.31 (.03)
Fundamentalist	−.21(−.05)	−.09(−.02)	.24 (.06)
Church attendance	−.18(−.21)*	−.21(−.24)*	−.13(−.16)*
R^2	.21	.23	.23
(*N*)	551	406	271

* Sig. at .05.

[a] Figures are unstandardized regression coefficients followed by betas in parentheses. Dummy variables representing the years of the survey were also included (i.e., a dummy variable for 1973 and 1974 in the first equation) but their effects were negligible and insignificant in each case.

Conclusions

Our findings revealed that black-white differences are still present on the abortion issue. While the overall gap between the two racial groups has narrowed since 1972, blacks were still less likely to support abortion than whites. Much of this difference, though not all, is due to the different demographic characteristics of blacks and whites, and the greater degree of religiosity (as measured by church attendance and strength of denominational and religious preference) of blacks.

Most of the factors hypothesized to affect black attitudes appeared to do so: education, income, urban and northern residence, and lesser religiosity were each related to greater support for abortion. We found no direct evidence to support or refute the legitimacy of the "abortion as black genocide" argument, but the fact that opposition to abortion comes from the seemingly most *traditional* segment of the black community (i.e., those who are more religious, less educated, from rural areas and the South) seems to provide some indirect evidence that that argument probably does not account for the greater black opposition to abortion.

Given the lower degree of support for abortion by blacks than whites, one might expect to see more blacks active in the various anti-abortion movements. However, two factors militate against this. First, the opposition from the black community tends to come predominantly from those groups who are the least articulate—the poorer, less educated, rural southerners. And it is likely that those blacks opposed to abortion who are highly articulate—church leaders, for example—are hesitant to identify themselves with the very conservative white-dominated anti-abortion movements, which in most cases are probably not particularly sympathetic to black concerns.

These findings are especially paradoxical in light of the fact that black women have proportionally more abortions than white women (Statistical Abstract, 1979). By 1977, the abortion rate per 1,000 women was three times as high for black (62.8) as white (20.9) women, and the rate per 1,000 live births was over twice as high (705 to 328). It is probable that the greater incidence of abortion among black women stems less from greater support for the idea of abortion and more from the need for the procedure itself. And, although we have no information on this, undoubtedly abortion is more frequent within those less traditional segments of the black community, those who are urban dwellers, for example, where support for abortion is higher than among the rest of the black community.[10]

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[10] Data on abortion are not categorized by race and region simultaneously, but these data do show that overall, abortions are more frequent in the North and the industrialized states. The abortion rate was highest in Washington D.C., Massachusetts, New York, California, Illinois, and Washington, and lowest in Arkansas, Idaho, Mississippi, Utah, and Wyoming.

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Public Attitudes Toward Life and Death

DARWIN O. SAWYER

HEATED controversy surrounds the legitimacy of life-taking actions in this country, with legal abortion and capital punishment heading a growing list of issues concerning the "right" to end (or prevent) human life. While past research has focused on specific issues in the area (e.g., Gelles, et al., 1975; Kalish, et al., 1974; Ostheimer, 1980; and Tedrow and Mahoney, 1979), the conventional wisdom is that individuals form generalized beliefs about such actions and that these belief systems are responsible for their attitudes toward specific issues. At present, there are two basic schools of thought on the nature and manifestation of these normative beliefs.

The first is a variation of the pro-life ideology sometimes attributed to anti-abortion groups (Granberg, 1978). In this view, public attitudes

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Abstract Opinion trends in this country indicate sharp divisions in public sentiment over a number of life-taking actions. While legal abortion and capital punishment clearly head the list, a number of other issues have gained national attention in recent years. The present paper explores the structure of belief systems giving rise to normative conflicts of this kind. Of particular interest is the notion of a "pro-life" or other generic life orientation (e.g., the alleged "right-to-die" orientation of those who favor "mercy killings" in the case of terminally ill patients) as a possible explanation for public attitudes toward specific issues such as suicide and euthanasia. The present analysis assesses the empirical claims associated with such a model. The results offer qualified support for the existence of generic value orientations as revealed by public attitudes toward legal abortion, suicide, euthanasia, and capital punishment.

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toward life and death are manifestations of latent beliefs about the morality of voluntarily ending a human life. While there are a number of variations on this basic theme, it is typically assumed that someone either favors or opposes the right to end human life, regardless of the circumstances surrounding the action. In other words, people are seen to be consistent in their attitudes toward specific issues and do not allow other considerations to color their basic life orientations.

As we will show in greater detail below, the pro-life hypothesis raises some interesting issues about public support for capital punishment. It also challenges a rival view which equates public support for the death penalty with popular oppositon to legal abortion and other, so-called liberal, issues such as mercy killings. More generally, this view assumes that public attitudes toward life and death are manifestations of underlying social values or political philosophies (Stinchcombe, et al., 1980). Thus, for example, while capital punishment is commonly viewed as a conservative cause, legal abortion is more often equated with a liberal value orientation.

While both models have aroused growing interest in recent years, there has been no systematic attempt to evaluate the existence of generic value orientations as a possible explanation for public attitudes toward specific issues in the area. As a step in that direction, the present paper formulates and tests a series of statistical hypotheses about the structure of public attitudes toward life-taking actions. The analysis revolves around the notion of a generalized belief system which transcends specific issues (abortion, suicide, etc.) and situational considerations (threats to a mother's life, terminal illness, etc.). Of particular interest is the extent to which such issues consistently divide the population into distinct normative groups. (Future work seeks to link these normative orientations to other social and political attitudes.)

Data and Methodology

The data for the analysis are taken from the 1977 General Social Survey conducted by the National Opinion Research Center at the University of Chicago. The survey is based on a national probability sample of the noninstitutional population in the continental United States 18 years of age or older in March or April, 1977.[1] Of the 1,530 respondents in the survey, 1,315 (or 89.9 percent) answered four questions dealing with various life-taking (or life-preventing) actions.

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[1] For present purposes, these data are treated as a simple random sample of the survey population. For further details, see NORC (1977).

As shown in Table 1, 64.0 percent ($N = 842$) said they approved of euthanasia "when a person has a disease that cannot be cured," 41.1 percent ($N = 540$) thought that "a person has the right to end his or her own life if this person has an incurable disease," 90.4 percent ($N = 1,189$) thought that legal abortion should "be available to a woman if the woman's own health is seriously endangered by the pregnancy," and 72.0 percent ($N = 947$) favored "the death penalty for persons convicted of murder."

All four of these items can be seen to involve a life-compromising situation of some kind. They also suggest tradeoffs of various kinds between alternate lives and alternate forms of death. The latter type of tradeoff is seen most clearly in the case of euthanasia and suicide, where the respondent is implicitly asked to choose between death by "natural causes" and death by "voluntary" action of some kind. The abortion item illustrates a variation on this general theme, depending in part on the respondent's beliefs about the onset of fetal life (and the imminent threat of maternal death should abortion not occur). That is, respondents may view abortion (as defined by the survey question) as either a life-taking or a life-preventing action. In either case, the implied choice is between the life of the fetus and the life of the mother. The capital punishment item raises still another set of issues concerning the valuation of human life. As presently phrased, an affirmative response to this item can be construed in at least one of two ways: as a life-taking and death-preventing action in the case of those who view convicted "murderers" as recidivistic or who believe in the deterrent effects of capital punishment; or as a life-taking (but not death-preventing) action in the case of those who share neither of these beliefs. (Additional research is needed to unravel the varying

Table 1. Percentage Distribution of Survey Responses to 4 Attitude Items Concerning Life and Death (N = 1,315)

Survey Item	*% Approving*	*% Disapproving*
Euthanasia[a]	64.0	36.0
Suicide[b]	41.1	58.9
Abortion[c]	90.4	9.6
Capital punishment[d]	72.0	28.0

SOURCE: National Opinion Research Center, 1977 General Social Survey.

[a] "When a person has a disease that cannot be cured, do you think doctors should be allowed by law to end the patient's life by some painless means if the patient and his family request it?"

[b] "Do you think a person has the right to end his or her own life if this person has an incurable disease?"

[c] "Should legal abortion be available to a woman if the woman's own health is seriously endangered by the pregnancy?"

[d] "Do you favor or oppose the death penalty for persons convicted of murder?"

elements in each situation. For present purposes, however, the analysis will be confined to the search for underlying patterns in the data as a means of motivating further inquiry along those lines.)

Table 2 provides a preliminary glimpse of the structure present in the data. As indicated by the gamma statistics in the lower half of the matrix, all relationships are positive in sign, thus suggesting a consistent pattern across the four items. With the exception of the abortion/capital punishment association, each relationship is moderate to large in magnitude as well. The top half of the matrix presents the goodness-of-fit chi-square statistic for each pair of items. Once again, all relationships are highly significant with the exception of the abortion/capital punishment one. Since the latter may reflect confounding influences of some kind, the apparent structure in the data clearly merits further investigation.

The following analysis is based on a model-fitting strategy proposed by Goodman (1974), which uses the independence hypothesis (i.e., the hypothesis of no association between two or more survey items) as a baseline for assessing the latent structure of a set of data. In this approach, the independence hypothesis can be viewed as a special case of the general model:

$$\pi_{ijklt}^{ABCDX} = \pi_{t}^{X}\, \pi_{it}^{\bar{A}X}\, \pi_{jt}^{\bar{B}X}\, \pi_{kt}^{\bar{C}X}\, \pi_{lt}^{\bar{D}X} \tag{1}$$

as applied to the four-way cross-classification of survey items in Table 1. In Equation (1), π_{t}^{X} denotes the probability that a randomly chosen respondent will be in the t-th latent class of variable X, $\pi_{it}^{\bar{A}X}$ denotes the conditional probability that a person favors (for $i = 1$) or opposes (for $i = 2$) euthanasia (under the designated circumstances), *given* that the person is a member of the t-th latent class; and $\pi_{jt}^{\bar{B}X}$, $\pi_{kt}^{\bar{C}X}$, and $\pi_{lt}^{\bar{D}X}$ denote the corresponding conditional probabilities for the suicide, abortion, and capital punishment items, respectively (for $j = k = l = 1$ when the respondent approves of the action and for $j = k = l = 2$) when the respondent disapproves).

Table 2. Measures of Association Between 4 Attitude Items Connoting Life-Ending or Life Preventing Actions[a]

	Euthanasia	*Suicide*	*Abortion*	*Capital Punishment*
Euthanasia	—	227.9	76.1	16.4
Suicide	.781	—	60.2	15.1
Abortion	.681	.787	—	1.5
Capital punishment	.328	.244	.120	—

[a] All statistics are based on the 2 × 2 tables formed by cross-classifying each pair of items in Table 1. Gamma statistics appear below the main diagonal. Corresponding chi-square statistics appear above the main diagonal.

Under the independence hypothesis, T (the number of latent classes in the model) = 1 and Equation (1) reduces to:

$$\pi_{ij\,kl\;t}^{ABCDX} = \pi_{ij\,kl}^{ABCD} = \pi_i^A\,\pi_j^B\,\pi_k^C\,\pi_l^D \tag{2}$$

Equation (2) expresses the expected probabilities (π_{ijklt}^{ABCDX}) in Equation (1) as the product of the A, B, C, and D marginals for Table 1. When the independence hypothesis is ruled out by the data, it is possible to increase the number of latent classes (i.e., T) incrementally until a satisfactory "fit" is obtained (subject to the number of cells in the original table). The end result is a T-class unrestricted latent structure model. (To aid in this model-fitting process, the parameters of the model can be *restricted* to conform to prior expectations about the latent variable in question. The resulting model also can be used to construct an index for classifying respondents in terms of their "latent class membership." The interested reader is referred to Goodman (1974) for further details on how this may be done.)

Results

Table 3 presents the cell frequencies for the four-way cross-classification of survey items in Table 1. The second column of numerical entries contains the expected frequencies under the baseline model of independence.[2] The goodness-of-fit (GFX^2) and likelihood-ratio (LRX^2) chi-square statistics for the independence hypothesis are 419.1 and 387.4, respectively. With 11 degrees of freedom, it is clear that the independence model does not fit the data in Table 3.

TWO-CLASS UNRESTRICTED MODEL

We will now consider a model that assumes the existence of two latent classes in the population. Unlike the independence hypothesis, where everyone is assumed to share the same belief system, public attitudes toward life and death are now viewed as manifestations of latent differences in value orientation between two distinct groups of individuals. (As shown below, the specific nature of each group is revealed by the conditional probabilities associated with the two latent classes). The third column of numerical entries in Table 3 contains the expected frequencies estimated under this two-class model. A row-by-row comparison with the observed frequencies in the first column of the table reveals a close correspondence to the sample distribution of response patterns. The GFX^2 and LRX^2 statistics for

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[2] All of the numerical results reported in this study were obtained from a computer program written by Clogg (1977; 1979b).

Table 3. Cross-Classification of Survey Items in Table 1

Response Pattern[a]				Observed Frequencies	Expected Frequencies Under	
E	*S*	*A*	*C*		*Independence Model*	*Two-Class Unrestricted Model*
A	A	A	A	370	225.1	369.3
D	A	A	A	41	126.5	43.2
A	D	A	A	251	323.1	250.2
D	D	A	A	200	181.5	204.7
A	A	D	A	5	23.9	4.9
D	A	D	A	4	13.4	3.7
A	D	D	A	23	34.2	18.6
D	D	D	A	53	19.2	52.6
A	A	A	D	99	87.5	99.7
D	A	A	D	19	49.2	15.6
A	D	A	D	86	125.6	87.1
D	D	A	D	123	70.5	119.2
A	A	D	D	1	9.3	1.7
D	A	D	D	1	5.2	2.1
A	D	D	D	7	13.3	10.6
D	D	D	D	32	7.5	32.0

[a] Where E = euthanasia, S = suicide, A = abortion, and C = capital punishment. An "A" in the body of the table denotes "approval" and a "D," "disapproval."

the model are 4.3 and 4.5, respectively (Table 4). With six degrees of freedom, the two-class model clearly fits the data very well, accounting for nearly 99 percent (1.0 − [4.5 / 387.4] = 98.8) of the variation in response patterns unexplained by the baseline model of independence.

EVIDENCE FOR A GENERALIZED LIFE ORIENTATION

Table 5 presents the parameter estimates for the two-class unrestricted model. The entries in the first column indicate the likelihood that a randomly chosen respondent will be in the first or the second latent class. The conditional probabilities to the right indicate the modal response pattern for each class. Two features of these estimates merit attention. First, approval rates are consistently higher for the first latent class. This is particularly striking for the first two items. Members of latent class one are nearly four times (.92 / .24 = 3.83) as likely to favor euthanasia under the stipulated conditions and (.66 / .06 =) 11 times as likely to favor suicide under similar circum-

Table 4. Goodness-of-Fit Statistics for the Life Orientation Model

Model	*DF*	*GFX^2*	*LRX^2*
Independence hypothesis	11	419.14	387.42
Two-class unrestricted	6	4.29	4.53

Table 5. Parameter Estimates for Two-Class Unrestricted Model

Latent Class	Latent Class Probability	Conditional Probability of Favoring: Euthanasia	Suicide	Abortion	Capital Punishment
One	.59	.92	.66	.99	.79
Two	.41	.24	.06	.78	.62

stances. Likewise, they are (.79 / .62 = .99 / .78 =) 1.27 times as likely to favor the death penalty *and* legal abortion.[3] The latter result is especially noteworthy, since it clearly challenges the notion that public support for capital punishment and legal abortion reflect distinctively different value orientations. The influence of sampling variations on these results can be assessed through the use of equality restrictions on the conditional probabilities for the model (i.e., by setting the conditional probability for item A, say, equal for both latent classes).[4] Table 6 presents the numerical results needed to evaluate the null hypothesis of no difference in approval rates between the two latent classes. The first two columns of the table report the GFX^2 and LRX^2 values under the restricted model for each item. Column three reports the differences in LRX^2 values between these models and the original, unrestricted model. In each case, the null hypothesis is rejected at the .01 level, thus ruling out the possibility that interclass differences in approval rates are the result of sampling error.

A second notable feature of the conditional probabilities in Table 5 concerns the modal responses associated with each latent class. A clear majority of those in the first latent class support euthanasia and suicide (92 percent in the case of the euthanasia item, 66 percent in the case of the suicide item), while a clear majority of those in the second latent class oppose both actions (100 − 24 = 76 percent in the former case and 100 − 6 = 94 percent in the latter). By the same token, a majority of respondents in *both* classes favor legal abortion *and* capital punishment, despite significant differences between

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[3] In cases where a parameter estimate approaches an extreme value such as .99, it is logical to test the null hypothesis that the true value is in fact 1.0. Failure to reject the null hypothesis is grounds for restricting the conditional probability to equal 1.0 in subsequent analyses. In such cases, there will be one more degree of freedom for testing the fit of the restricted model.

[4] In more formal terms, we are imposing equality restrictions on successive pairs of conditional probabilities while allowing the other conditional probabilities to vary freely. This procedure amounts to a test of the null hypotheses that $\pi_{i1}^{\bar{A}X} = \pi_{i2}^{\bar{A}X}$ in the case of the euthanasia item, that $\pi_{j1}^{\bar{B}X} = \pi_{j2}^{\bar{B}X}$ in the case of the suicide item, that $\pi_{k1}^{\bar{C}X} = \pi_{k2}^{\bar{C}X}$ for the abortion item, and that $\pi_{l1}^{\bar{D}X} = \pi_{l2}^{\bar{D}X}$ for the capital punishment item.

Table 6. Chi-Square Tests of the Null Hypotheses of No Differences in Approval Rates

Null Hypothesis Pertaining to:	GFX^2	LRX^2	$LRX^2(H_0)-LRX^2(H_A)$
Euthanasia	276.1	299.6	295.1
Suicide	258.3	285.5	281.0
Abortion	99.0	106.1	101.6
Capital punishment	33.6	33.1	28.6

classes in relative approval rates. These findings raise several questions about the existence and nature of a generalized life orientation.

THE ROBUSTNESS OF THE LIFE ORIENTATION MODEL

In a recent study of popular opposition to legal abortion, Granberg (1978) rejects the notion of a "generalized pro-life position" in this country. The author's conclusions are based partly on data from the 1975 General Social Survey (GSS) and on comparably worded questions for two of the items included in the present study. However, Granberg's approach differs sharply from the one adopted here. Using a summated scale of abortion support,[5] the author reports little or no relationship between scores on this scale and a respondent's attitude toward the death penalty and several other issues thought to indicate a generalized life orientation.[6] A number of other studies have challenged the unidimensional model of abortion attitudes on which these results are based (e.g., Arney and Trescher, 1976; Blake, 1971; Clogg and Sawyer, 1981; Westoff, et al., 1969). Thus, it is conceivable that Granberg's findings are an artifact of the scaling procedure used to measure abortion attitudes. Coupled with the present results, however, they do point up the need to consider the construct validity of the life orientation model.

The results in Table 7 shed additional light on this issue. The six items in the first column are references to the abortion questions analyzed by Granberg and included in the 1977 GSS. The first question (denoted "health") is the one analyzed above. The remaining

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[5] Granberg's (1978) study is based on a scalogram analysis of six abortion items. While acknowledging published evidence of bidimensionality in these data, the author proceeded to construct a summated scale of abortion support, justifying this decision on the basis of conventional "rules-of-thumb" for assessing the scalability of response patterns. Recent research (Clogg and Sawyer, 1971) has confirmed the bidimensional structure of these data and seriously challenged the viability of such diagnostic tests as item "scalability" and "reproducibility."

[6] These other items included "confidence in the military" and opinions regarding "increased military spending" and an "expansion of the effort to improve and protect the health of the people." While we would quibble over the face validity of these items as indicators of a generalized life orientation, we concede that the 1975 GSS did not contain either of the other items analyzed in the present study.

Table 7. Goodness-of-Fit Statistics and Parameter Estimates for 6 Variations on the Life Orientation Model

Abortion Item	*GFX²*	*LRX²*	*Approval Rates*	
			Latent Class One	*Latent Class Two*
"Nondiscretionary"				
Health	4.3	4.5	.99	.78
Rape	5.0	5.0	.96	.66
Defect	7.8	9.4	.96	.67
"Discretionary"				
Single	16.0	16.2	.72	.24
Poor	7.0	7.1	.77	.25
Nomore	21.0	21.6	.72	.22

questions asked the respondent whether legal abortion should be available to a woman "if she became pregnant as a result of rape" (denoted "rape"), "if there is a strong chance of serious defect in the baby" (denoted "defect"), "if she is not married and does not want to marry the man" (denoted "single"), "if the family has a very low income and cannot afford any more children" (denoted "poor"), and "if she is married and does not want any more children" (denoted "nomore"). In related studies (e.g., Westoff, et al., 1969), these items have been found to form two separate patterns, the first three suggesting "medically justifiable" reasons for abortion and the other three suggesting extenuating circumstances used to justify abortion on "discretionary grounds." In view of these differences in item content, we would expect the "rape" and "defect" questions to yield results more closely approximating the ones reported for the "health" item. The chi-square statistics and parameter estimates in the body of Table 7 seem to bear this out.

Each new item in Table 7 was first cross-classified with the euthanasia, suicide, and capital punishment items described above, thus yielding five additional tables of cell frequencies comparable to Table 3. Each of these tables was then analyzed using the unrestricted two-class model. The GFX^2 and LRX^2 values for the five models are reported in the first and second columns of Table 7. As before, there are six degrees of freedom for testing the statistical significance of each model. The third and fourth columns of Table 7 give the conditional probabilities of approval for the two latent classes. In all five cases, the basic pattern for the other conditional probabilities in the model is the same as the one reported in Table 5.

A brief overview of Table 7 reveals two noteworthy features of the basic model. First, the statistical fit remains rather robust under successive alterations in item content. As we were led to expect by the "discretionary-nondiscretionary" dichotomy, the "rape" and "defect" models closely rival the goodness-of-fit qualities displayed by the earlier model. Likewise, except for the "poor" model, the discretionary items result in a somewhat poorer characterization of the data. Turning to the estimated approval rates in Table 7, we note similar correspondences among the structural parameters for the first three models, with those based on the discretionary items diverging noticeably in common directions. On both accounts, the results in Table 7 seem to support the notion of a generalized life orientation, at least in the restricted sense of the term noted above (i.e., with consistently differing probabilities of life-supporting attitudes between the two latent classes.)

Conclusion

Opinion trends in this country indicate sharp divisions in public sentiment toward a number of life-taking (or life-preventing) actions. While legal abortion and capital punishment clearly head the list, a number of other issues have gained national prominence in recent years. For good or ill, it would appear that such issues are rarely if ever resolved, at least in the sense of reaching popular consensus on appropriate courses of action. In the present paper, we have explored the structure of belief systems sometimes thought to give rise to normative conflicts of this kind.

The analysis has focused on the notion of generic value orientations as an explanation for public attitudes toward specific questions of life and death. Contrary to popular conceptions about legal abortion and capital punishment, individuals display a definite strain toward consistency in their attitudes toward life-taking and life-supporting actions. At the same time, our results stop short of supporting the more extreme notion of a "pro-life" or "right-to-die" orientation. Thus, while one segment of the population is consistently more likely to approve of all four types of life-taking action, a majority in both groups favors legal abortion as well as capital punishment. It would seem, then, that certain issues (namely, euthanasia and suicide in the present context) are more discriminating than others in terms of their ability to differentiate among divergent belief systems. The fact that someone opposes the death penalty, for example, does not imply opposition to legal abortion as well, although it does suggest greater tendencies in that direction. In this respect, the present findings should prove useful in determining which issues are more likely to reveal a person's latent value orientation as well as in inferring individual attitudes toward specific issues.

They also raise a number of research questions of use in assessing the broader implications of the present study. For example, it would be useful to know how the basic model behaves when other items are included in the analysis. Preliminary findings on this point were discussed above. In the case of abortion attitudes, the model was found to conform to prior expectations, despite successive alterations in item content. In principle, there is no reason why the same sort of tests could not be carried out with still other items. In the case of the suicide item, for example, the 1977 GSS also asked respondents whether they thought "a person has the right to end his or her own life if this person has gone bankrupt," "dishonored his or her family," or "is tired of living and ready to die." At present, however, it is premature to follow this line of analysis, since too little is currently known about the internal structure of these items to guide the selection of appropriate test procedures. Unfortunately, in the case of the other items, the GSS contains no comparable information of this kind. It is hoped that the preceding analysis will help stimulate additional work along both lines.

A related issue concerns the interpretations attached to the basic model. Throughout, we have emphasized inter-item variations in situational content. For example, in the case of the euthanasia and suicide items, we argued that the respondent was implicitly being asked to choose between alternate ways of dying. Similarly, we said that the abortion and capital punishment items could be construed to suggest a tradeoff between alternate lives. More generally, all four items implicitly contrasted situations where death occurs as a result of voluntary intervention rather than "natural causes." To some, these ambiguities may reflect defects in survey design (particularly, item wording). By contrast, we see them as offering a promising direction for future research by acknowledging the true complexities inherent in such situations. If there is a problem, it is that the present items do not provide enough specificity to isolate all relevant variations in situational content. Thus, of the three contrasts just enumerated, only the third clearly encompasses the response consistencies actually found to exist. For this reason, we recommend that future survey questions be formulated to facilitate direct comparisons across other situations as well, so that it will eventually be possible to assess the relative contributions made by a number of different elements without forcing situations to conform to as many simplifying assumptions.[7]

[7] A related concern is the practical issue of scaling response patterns as a preliminary step in subsequent analyses of the data (e.g., in attempting to uncover the causal mechanisms responsible for a person's latent value orientation). In describing the latent structure approach, a person's class membership was said to be inferred from his or her

A final issue concerns the number of latent variables needed to describe the data. Coupled with the fact that each latent class exhibits a consistent *pattern* (in relation to the other class) of responses on all four survey items (i.e., one class being consistently more likely than the other to approve of each life-taking action), the present findings might seem to rule out other possible orientations toward life-taking actions. While we have emphasized the need to examine other issues and situations, the approach we followed indicated no evidence of additional dimensions in the data. Still, it should not be concluded from this that our results preclude the need for further work along these lines.

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response pattern on the four survey items. Although, intuitively, it may seem to be a simple matter to determine who belongs to which latent class, in actual practice the issue is usually more complicated, since the relevant parameters describe statistical tendencies rather than deterministic relations. Associated with every response pattern is a known set of estimated probabilities relating that response pattern to each latent class. Unfortunately, from a scaling perspective, these probabilities are seldom either zero or one for all response patterns. As a result, it is rarely possible to classify respondents with complete certainty. While this complicates scaling procedures, it is in no way unique to the present approach. Moreover, the problem is actually quite manageable given certain assumptions about the behavior of response errors in the data. We will not pursue the matter further at this point, except to note that appropriate scaling procedures currently exist and appear to yield rather reasonable results (e.g., see Goodman, 1974).

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Topic Variation in Levels of Agreement Between Parents and Adolescents

DOROTHY JONES JESSOP

WHAT is the extent of agreement between high school students and their parents in their testimony about each other and their relationship? This study is an analysis of independent reports on identical matters from high school students and their parents. Both parents and students were asked about individual behaviors and attitudes (parent and student use of and attitudes toward drugs, parent educational aspirations for the adolescent, and student church attendance and TV viewing), as well as family interaction and relationships, and sociodemographic characteristics. The object of this paper is to ex-

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Abstract The object of this paper is to examine why agreement between parents and adolescents varies across topics in the way it does and why discrepancies take the particular form they do. Data from a large-scale survey of adolescents matched with independent reports from their parents ($N=3{,}988$) have been used to show that the level of dyadic agreement is influenced by the objectivity of the topic, its relation to social norms, and its salience to the respondents. Agreement is high when the topic is objective and unambiguous, and when it does not activate problems of social desirability or personal threat. Topics which refer to recent events produce slightly less agreement than less timebound topics when the topic is a potentially threatening one. The level of agreement does not change if the referent is changed from the parent, to the student, or to those outside the dyad.

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plain why intradyadic agreement varies across topics in the way it does and why discrepancies take the particular form they do. Methodologically, it is important to know areas in which the report of one family member can be used as a surrogate for the report of another family member without affecting the relationships among variables. Substantively, agreement in the description (and, presumably, perception) of behaviors and attitudes is important because the child's adoption of a parental attitude or behavior may depend in part upon whether the child correctly perceives the parental position (Furstenberg, 1967; Kandel, 1974; Niemi, 1968; Tedin, 1974).[1]

Agreement can vary with the psychological traits of the individuals in the dyad or the quality of the relationship, but mainly these are relevant through the way they relate to the substantive *topic* on which dyad members report (Jessop, 1979). Variations in agreement and disagreement with the nature of the topic have been suggested previously by the work on: (1) group dynamics, with its distinction between perceptual agreement on objective reality and perceptual agreement in situations in which no such criterion exists (Brown, 1965); (2) interobserver reliability of survey and clinical reports, with the distinction between agreement on "hard" and "soft," objective and subjective, and behavioral and attitudinal data (Ballweg, 1968; Singer, 1973; Sudman and Bradburn, 1974; Mechanic, 1978); (3) disparate surveys of parents and children, most of which individually looked at only a single or a few related topics at a time (e.g., Furstenberg, 1967; Kandel and Lesser, 1968; Larson, 1974; Mason et al., 1976; Niemi, 1974; Oliver, 1977; Rossiter and Robertson, 1975; St. John, 1979; Tedin, 1974) and which collectively reveal wide intertopic variation in levels of agreement.

The survey literature concludes that dyadic agreement is *lower* on less objective topics, threatening topics activating social desirability biases and problems of self-presentation, and topics removed in time or importance from respondents' experience (Sudman and Bradburn, 1974; Cannell, Oksenberg, and Converse, 1977). The group dynamics literature, on the other hand, suggests that agreement between subjects is *higher* when there is no objective standard (Tedeschi and Lindskold, 1976:551–52), and the attribution literature suggests that an observer is better able to grasp and report on a situation than an actor oriented to the cues necessary for further action (Jones and

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[1] This paper examines whether the survey reports of matched dyads of parents and adolescents *agree* in their descriptions of certain behaviors, attitudes, family interactions and relationships, and sociodemographic characteristics within their families. Other analyses of the same data focus on the question of *intergenerational influence* within the dyads (Kandel, 1974).

Nisbett, 1971). All bodies of literature seem to agree that personal history and internal states are known best by the actor.

Based on these findings the present hypotheses are as follows:

1. Items which are objective and unambiguous to both dyad members will produce high agreement. Objective material is that on which, in principle, it is possible to collect an account from another person as verification. A topic is unambiguous if the categories used to define it are clearcut and well-defined, and do not require a high degree of inference on the part of the one reporting (Frick and Semmel, 1978).

2. Items which are nonproblematic normatively for both dyad members will produce high agreement. The essence of social desirability is that one response category is distinctly more socially approved than the other option(s). Sometimes the difference in the normative valuation is negligible, while in others it is large; in some cases none of the categories are clearly illegal, while in others a category involves clearly illicit behavior. The relevance of the topic to social norms indicates whether possession of a trait is positively sanctioned, negatively sanctioned, or of no interest normatively, i.e., the degree to which a deviance/conventionality dimension is involved at all. A related aspect is the degree of sociocultural conflict over the items. A topic not the subject of conflict may be more ambiguous because it is not questioned or examined. The hypothesis is that in the present data normatively problematic topics with one or more socially desirable responses and controversial topics will produce lower levels of dyadic agreement than topics which do not have this social desirability component and are noncontroversial.

3. Salience interacts with the relation of a topic to social norms (Sudman and Bradburn, 1974:53–54). For topics which are not normatively problematic, highly salient items will produce greater agreement than items with low salience. For topics which are normatively problematic, highly salient items will exhibit less dyadic agreement than will low salience items because of the personal threat involved.

4. The referent of the topic also will interact with the relation of the topic to social norms in such a way that personally threatening items (recent and involving dyad members) will produce less agreement than nonthreatening items.

A second intent of this paper is to examine why, in the absence of dyadic agreement, discrepancies take the particular form they do. It is hypothesized that low levels of agreement will be associated with disagreements systematically skewed according to the personal characteristics of those reporting and the characteristics of their relationship (i.e., ambiguity and lack of objectivity, problematic relation to social norms).

In Table 1 the four theoretical dimensions are schematically presented and concrete items are categorized. Specifically, the hypothesis is that as sociodemographic characteristics are more objective and less ambiguous than behaviors, and behaviors more objective and less ambiguous than attitudes, the level of agreement will be greater on sociodemographic characteristics than on behaviors, and lowest on attitudes. Family interactions (behaviors) and relationships (more akin to attitudes) will overlap individual behaviors and attitudes, with reports of family interaction having levels of agreement similar to individual behaviors, and reports of family relationships having levels of agreement similar to individual attitudes. Correlatively, dyadic disagreements will be more systematically patterned for attitudes than for behaviors, and more for behaviors than for sociodemographic characteristics.

Sample and Data

The data come from a large-scale study of family and peer processes in adolescent drug use carried out in 1971–72 (Kandel, et al., 1976). Structured questionnaires were completed in 18 schools by a representative sample of New York State secondary students ($N=$ 8,206) and parallel mailed questionnaires were completed by 5,574 (61

Table 1. Levels of Interobserver Agreement (Weighted Kappa) in Parent/Student Dyads (N ≥ 2,157) by Type of Topic

	Objectivity							
	High		*Medium*			*Low*		
	Sociodemographic Characteristics		*Individual Behaviors*		*Family Interactions*	*Family Relationships*	*Individual Attitudes*	
Norm Relevance	*Parent*	*Student*	*Parent*	*Student*			*Parent*	*Student*
	Most Demographics		*Non-Drug-Related*					
Nonproblematic								
Median	.80	.98		.52	.20	.17	.44	
Range	.70–.89	—		.35–.68	.16–.24	.16–.24	—	
N of topics	4	2		2	12	12	1	
	Sensitive Demographics		*Drug-Related*					
Problematic								
Median	.48		.27	.07	.17	.07	.18	.00
Range	—		.21–.81	.01–.28	.15–.24	—	.16–.20	−.01–.04
N of topics	1		10	10	4	1	4	9

percent) of their parents. Self-generated identification numbers were used to match an individual parent and his/her child. The resulting relational sample of parent-adolescent dyads (N=3,988) is the basis of the present analysis. The sample and data collection procedures are described in detail elsewhere (Kandel, 1973:1067).

Students and parents who provided usable codes and could be matched to each other differ in significant ways from the original representative sample from which they were drawn. Those not providing usable codes are more likely to buy and sell drugs and to have friends who use marihuana. Students who provide a code are among the better students and are more likely to be girls, to come from intact families, and to attend church more frequently. Though there is no difference between the two groups in family income, those giving codes come from better educated families and are more likely to be white than black or Puerto Rican. In sum, students providing a code seem to be the more conforming students, and perhaps those most respectful of authority (Marel, 1973). This might suggest that the levels of agreement obtained herein are higher than those that exist in the general population of families. However, the greatest differences between those included and excluded were in drug variables and there seemed to be no differences between included and excluded respondents in family attitudes or psychological variables. This might lead us to expect that the differences between these results and what would be obtained from a truly representative relational sample would be differences most specifically in deviant behaviors and attitudes rather than differences pertaining to family life.

The parent and student questionnaires were parallel instruments asking about a variety of sociodemographic characteristics, patterns of use of licit and illicit substances, attitudes toward drugs, family interactions and relationships, relationships with peers, intrapsychic states, attitudes and values. The 67 questions asking parents and students to give a report on the identical phenomenon comprise the data for the present analysis.

Method

Parent and student reports of a given phenomenon were cross-tabulated. These tables were summarized with a variety of statistics, each of which elucidates a slightly different aspect of parent-adolescent agreement. The marginal distributions of parent and adolescent responses indicate something about generational differences. The percentage of dyads agreeing is the sum of the dyads falling along the major diagonal of a table. It does not take into account the total configuration of the table, the relative severity of different disagreements, or chance agreement based on similar marginal distributions. It depends heavily on the number of categories used and largely reflects the marginal distributions of the two variables. On the other hand, the percentage agreeing indicates instances in which the same response would be obtained if either the parent or the adolescent is the source of data, and tells something about the climate within the family. The distribution within disagreeing dyads, the percentage of those dyads off the main diagonal in which the parent gives a more "positive" response than the adolescent (as defined by the categories listed in Appendix A), is likewise dependent on the marginal distributions of the component variables.

In order to summarize the strength of the relation between the parent and the adolescent reports in a form relatively free of these influences, weighted kappa was used (Fleiss, 1973). Kappa is a measure, not of association or correlation, but of the degree of category-for-category agreement which exists beyond that which could be attributed to chance alone. Kappa was developed primarily to measure the reliability of two or more raters' categorizations of a subject, particularly psychiatrists' ratings of patients in a clinical setting. Like tau-beta, kappa takes the total cross-tabulation into account, ranges from −1.0 to +1.0, and has a derivable standard error. For ordinal level variables a one-category difference between two reports is less serious than a three-category difference. Therefore, weights assigned to cells of a table depend on how far they are off the major diagonal (Fleiss, 1973:143–147).[2] Kappa can be used with ordinal data, and can be generalized to more than two raters. It thus has the advantages claimed for another measure of agreement, Robinson's A (Robinson, 1957, 1959), and, in addition, it has a direct test of significance and makes less restrictive assumptions regarding the level of measurement and the shape of the distribution.

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[2] Much of the related literature on intradyadic agreement has used tau-beta, a measure of the degree to which the dyadic reports are a function of each other. To facilitate comparison with earlier work we calculated tau-betas and compared them with kappas. In no case would the use of kappa change the interpretation of the results. In virtually all cases, the kappa obtained is just slightly lower (by .00 – .05) than the tau-beta. In only 2 of 67 instances was the difference between the two coefficients even as large as .10. The questions on which the largest discrepancies were obtained are topics for which there were only two categories, very skewed marginals, and considerable discrepancy between the parent and student distributions relative to that obtained on other topics. While conceptually kappa is a more meaningful statistic when studying agreement, it would seem for all practical intents and purposes not to make any difference which of the two statistics is used.

Results

LEVELS OF DYADIC AGREEMENT BETWEEN PARENTS AND ADOLESCENTS

It is not surprising that parents and their adolescent children see some things differently and some things in the same way — the figures presented in Table 2 and Appendix A cover a range from almost perfect agreement to no agreement at all, depending on the topic.

OBJECTIVITY AND LACK OF AMBIGUITY OF THE TOPIC

It seems clear from the figures presented in Table 2 and Appendix A that levels of agreement vary with the objectivity and lack of ambiguity of the topic. Agreement on sociodemographic characteristics is high (.48–.98); agreement on attitudes (.00–.44) and family interactions and relationships (.07–.24) is low. Agreement on individual behaviors exhibits the greatest variation (.21–.81) with the topic. While ranges overlap there is a clear gradient from high agreement on the most objective and unambiguous topics to low agreement on the more subjective.

Table 2. Distribution of Percent of Dyads in Which Students Report More Positively Than Parents by Type of Topic[a]

	Objectivity							
	High		*Medium*			*Low*		
Norm Relevance	*Socio-demographic Characteristics*		*Individual Behaviors*		*Family Interactions*	*Family Relationships*	*Individual Attitudes*	
	Parent	*Student*	*Parent*	*Student*			*Parent*	*Student*
	Most Demographics		*Non-Drug-Related*					
Nonproblematic								
Median	59	48		58	25	40	46	
Range	53–60	—		44–61	16–56	33–48	—	
N of topics	3	2		2	12	3	1	
	Sensitive Demographics		*Drug-Related*					
Problematic								
Median	44		56	96	36	14	44	15
Range	—		43–73	76–98	28–44	—	19–70	1–60
N of topics	1		10	10	4	1	4	9

[a] The number of dyads in which there is disagreement ranges widely ($N = 3{,}037$), varying inversely with the amount of agreement (Table 1 and Appendix A). All are in excess of 100 with the exception of student grade ($N = 97$) and student sex ($N = 42$).

THE RELEVANCE OF THE TOPIC TO SOCIAL NORMS

Within each of the broad classes of topics (sociodemographic characteristics, individual behaviors, family interactions and relationships, and individual attitudes) there is a general gradient. Agreement is higher on items which are less likely to activate social desirability concerns, and lower on those which are more problematic (see Table 2 and Appendix A). (Looking *within* a class of topics is roughly equivalent to holding constant the level of objectivity of the phenomenon.)

Questions pertaining to drugs produce lower agreement than questions not related to drug use. The level of agreement is lower for conversation about "drugs" than for other types of conversation, and agreement over rules about "drugs" is lower than for other rules. We interpret this to indicate that the term "drugs" implies illicit drugs, the use of which is very problematic normatively.[3]

The degree of agreement regarding *specific types of substances* varies widely, however. It extends from the highest to the lowest for each class of topic and parallels the social acceptability of the substances, the prevalence of use of the substance, and perhaps the visibility in use of the substances (Appendix A).

1. Levels of agreement about *parental drug use* parallel from high to low the Guttman scale patterns of the ordering of substances from most frequently used to least frequently used substances.[4] As described previously for these data (Kandel, 1974), the greatest agreement occurs over socially acceptable substances used in a social context (cigarettes, .81; beer/wine, .54; and hard liquor, .50). There is lower agreement about pill use (tranquilizers, .44; amphetamines, .37; and barbiturates, .30), and drastically lower agreement about parental marihuana use (.21). The ordering of the agreement *among* the socially used substances and *among* the pills suggest that the effect of social acceptability may be stronger than the effect of visibility. Agreement on use of tranquilizers is higher than that on amphetamines and barbiturates. The fact that agreement on smoking is

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[3] This was the finding of a survey conducted by the National Commission on Marihuana and Drug Abuse (1973:10) in which respondents were asked to indicate which substances are subsumed under the term "drugs." The survey, which was conducted at approximately the same historical point as the survey from which the present results are drawn, found that both adults and youths overwhelmingly tend to regard heroin, marihuana, cocaine, the amphetamines and barbiturates as drugs. Substances such as alcohol and tobacco are generally not considered drugs.

[4] Guttman scale analyses of student use indicate the following ordering: any legal drug, cannabis, pills, psychedelics, cocaine, and heroin (Single et al., 1974). The assumption here is that the more widely used substance is more socially acceptable.

so much higher than on alcohol use may suggest either the greater visibility of smoking done on a daily basis at home or the less problematic nature of smoking as compared to drinking.

2. The level of agreement on the presence of a *parental rule* regarding smoking (.24), drinking (.18), and drugs (.16) varies positively with the social acceptability of the substances (Appendix A).

3. For *student drug use* there is a general correspondence between the acceptability of the substance and the level of agreement: hard drugs produce the lowest level of agreement, and agreement is slightly higher for soft drugs and alcohol.

There is one unexpected and large discrepancy: student marihuana use produces more agreement (.28) than student hard liquor use (.16). Similarly, parental attitudes toward marihuana produce slightly greater agreement than those toward alcohol use. These discrepancies reflect the fact that in these reports generational differences are greater over the question of alcohol than over any other substance. Sixty-four percent of students but only 20 percent of parents indicated that the student had used alcohol. In contrast much lower and more similar percentages of parents (6 percent) and students (27 percent) report use of marihuana. The lower level of agreement on alcohol may also be attributable to the greater variation in pattern of alcohol use. Making a guess about an activity or trait of another is more difficult if the activity is evenly split in the population of similar individuals than if the trait is extremely skewed. For substances used by 1–3 percent of adolescents a parental guess of nonuse would be virtually always right.

It was hypothesized that agreement would be lowest on topics which were normatively problematic for one or both dyad members. The findings and further reflection, however, would seem to indicate that it is in the *nonsymmetrical situation*, in which the topic is problematic for one member of the dyad but not the other, that agreement is lowest. While there is no way of checking this in the present data we hypothesize that when a topic is problematic for both dyad members, agreement will be produced because *both* dyad members will modify their reports and deny the problematic traits.

SALIENCE

It was hypothesized that topics pertaining to currently ongoing events, which are more salient, would produce greater agreement than less recent topics if the topic is not normatively problematic, and less agreement if the topic is normatively problematic. A test of this hypothesis exists in the comparison of agreement on whether a substance was ever used and whether the substance is being used currently. As the substance is the same, its social acceptability is held constant. In every case a question of whether a substance has been currently used produced lower agreement than the question of whether it was ever used (see Appendix A).

REFERENT

A further hypothesis was that the level of dyadic agreement would be higher if the topic referred to the parent and/or student than if it referred to neither of them. Marihuana use is the one topic asked with respect to three referents: (1) the parent's use, (2) the student's use, and (3) the percentage of students in the child's grade who have tried it. Additionally, marihuana is equally illicit for all three referents. The referent does not seem to make any difference for the level of agreement: parent ever-use of marihuana (.21), student ever-use of marihuana (.28), and percentage of students in grade using marihuana (.25) (Appendix A).

INTERCATEGORY DIFFERENCES IN AGREEMENT

It is being claimed that the level of dyadic agreement varies with the objectivity and lack of ambiguity of the topic, its relation to social norms, and its salience. Further support for this argument can be garnered from an examination of the categories of each variable which produce the greatest and least amount of dyadic agreement. If dyad members agree most often on categories which insight would suggest are normatively problematic, ambiguous, or lack objectivity, the argument is weakened considerably.

Variation in the category-specific kappas[5] presented in Appendix A suggests that agreement is higher for socially acceptable responses. The terminal rounding which Niemi (1968) and St. John (1979) found in reports of education levels seems applicable to a wide variety of topics, indicated by a preference for picking clearly demarcated and well-defined alternatives, often those at either end of a distribution. Thus intercategory differences support the pattern of intertopic differences previously found.

DIRECTION OF DISAGREEMENT

The second purpose of this analysis was to explain why discrepancies take the particular form they do. This is indicated by the percentage of disagreeing dyads in which the student gives a more positive response than the parent. (The "positive" response designated in Appendix A is usually considered the presence of the characteristic, behavior, or attitude.) Disagreements are "randomly distributed" if approximately 50 percent of the students give more positive responses than the parents. "Systematically distributed disagreements" occur, on the other hand, when the student gives a more positive response than the parent in far lower (e.g., 30 percent) or far greater (e.g., 90 percent) percentages of the disagreeing dyads.

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[5] The kappa for each category can be interpreted as an intraclass correlation coefficient (Fleiss, et al., 1969) and can be used as an estimate of which of the categories is producing the most (or least) difficulty for the raters.

The general hypothesis was that situations producing low agreement also produce more systematic distributions of the disagreeing dyads. But as Table 2 and Appendix A indicate, this does not seem to be the case; ambiguous topics can produce low agreement along with a random distribution of disagreements. A *problematic relation to a social norm* present in the form of social desirability or personal threat is needed to produce a systematic pattern of disagreements.

Response effects are often discussed in terms of a social desirability bias which seems to imply one social norm operative for all. Parents' and students' reports clearly indicate that this is not the case. Systematically distributed disagreements occur in instances in which parent and student norms differ. If respondents bias responses in socially desirable directions, the norm operative for each is that of his/her own role.

Concretely the distributions in Table 2 and Appendix A indicate that disagreements are relatively evenly balanced for sociodemographic characteristics, nondrug behaviors, and the nondrug attitude (parent educational aspirations for the student). The minor differences in these areas are in the expected directions: parents report higher family income, more frequent church attendance by the student, and higher educational aspirations for the student than do students; students frequently report more time spent watching TV than do parents.

It is in the areas of drugs and family life that the most systematic patterning of disagreements is seen. Disagreement over drug use takes the form of the student indicating use (or more frequent use) not reported by the parent, as noted earlier by Kandel (1974). The most extreme distribution of disagreements is on the question of student use of a variety of illicit substances: in all except one instance 89 percent or more of the disagreeing dyads consist of a student indicating use not reported by the parent.

When parents and students disagree regarding family life, on the other hand, it is more likely to be the parent who gives the more positive response. Parents are more likely than their children to indicate that they have a strong role in guiding and making decisions with their children, that rules exist about a variety of topics, and that they talk with their children frequently. Disagreements over the degree of parent/student closeness, however, are more evenly distributed. The differences seem limited to the areas of power and self-determination rather than reflecting a general conflict of perspectives over the affective areas of family life (Jessop, 1981). Differences with respect to decision making and parental influence, rather than manifesting a general parental attempt to portray a positive image of family life, more specifically reflect a tendency for each family member to enlarge his/her own role in family affairs.

More severe discrepancies occur over topics considered serious *by one or the other* but not by both. The most problematic topics produce *less severe* distributions, because they are reported in a more similar manner by *both* parents and their children. For example, much higher percentages of students than of parents indicate student use of each substance, but the percentage of students indicating use of heroin (1 percent) becomes much smaller and equal to that of the parents (1 percent). Thus, the balance of disagreements is less severe over student use of heroin (76 percent) than over use of substances which would be regarded less seriously by one or both members of the dyad. Similarly, higher percentages of parents than of students indicate most rules exist, but it is on a question of the most serious subject, drugs, that the percentage of students indicating there is a rule becomes much larger (74 percent) and closer to that of the parents (77 percent). In sum, the most severe discrepancies occur not over the most socially sanctioned items but over topics *differentially* problematic for parents and children, those involving norms changing historically or over the lifecycle.

Summary and Conclusions

Data from a large-scale study of adolescents and their parents have been used to analyze topical sources of variation in the degree of agreement in testimony by family members. The data have shown that the level of agreement is influenced by the degree of objectivity of the topic, its relation to social norms, and its salience to the respondents. Agreement is higher when the topic reported on is objective and unambiguous, and when it does not activate problems of social desirability or personal threat. Topics which refer to recent events produce slightly less agreement than less timebound topics when the topic is a potentially threatening one. The levels of agreement do not change if the referent of a given topic is changed from the parent, to the student, or to those outside the dyad.

Discrepancies between the reports of parents and students were such that the response of each dyad member was in the direction of the norms of his/her role. The data suggest that reports are brought in line with role-specific norms rather than a general social norm, and thus discrepancies would be most severe between persons occupying roles with conflicting norms. This supports previous literature. For example, Clark and Tifft (1966:522) found "strong evidence that errors on questionnaires are directly associated with perceived discrepancies between individual acts and personal and group norms. The errors represent an attempt to make reported behavior compatible with perceived norms." The evidence here indicates that the respondent also brings reports about other persons into line with the norms of the respondent's role.

Analyses such as these can indicate areas in which the report of one respondent can be used as proxy for that of another. The data seem to indicate that nonthreatening sociodemographic characteristics and nonthreatening activities of the self or of another can be accurately reported by a dyadic partner. Threatening behaviors, all subjective attitudes, and interaction and relationships in the dyad are not well reported by a proxy or surrogate. The data also suggest, however, that deciding what is threatening or nonthreatening is not a simple matter. It would have been a mistake, for example, to assume that reports of all psychotropic substances are equally threatening. Each respondent's perspective has to be assumed to determine what is threatening for that role.

If the object of study requires data not attained adequately from a proxy, a relational sample may be needed. If it is not feasible for the entire sample to be relational, it may be possible to augment a basic sample with at least a subsample of relational partners to gain estimates of the nature and direction of differences in response. What will not suffice is to try to estimate the degree of consensus in the dyad solely through the reports of a single respondent. Just as surveys of parent/child influence using one respondent overestimate the amount of generational continuity, surveys using one respondent for reports of others' attitudes and behaviors will overestimate the similarity between the two (Kandel, 1974).

Individual survey reports remain individual reports with serious limitations as representations of group level properties. While low agreement over individual attitudes may indicate that it makes little sense to ask for reports of personal attitudes from a third party, we have no simple solution as to whose report of dyadic or group interaction and relationships to accept. One family member's report of the quality of family relationships cannot be reified as an adequate or complete rendition of family relationships. Each family member's report remains a partial representation, which should be carefully maintained by the labeling of the variables and the use to which they are put in analysis. The decisions as to whether there is a need for reports by both parties, whether they should be fitted together in a typology, or whether they should each be retained in an analysis have to be clearly thought through in light of the substantive interests of a particular analysis.

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Appendix A. Variables and Categories on Which Parents and Adolescents Gave Independent Reports: Kappa, Response Categories, Marginal Frequencies, and Distribution of Disagreements (N ≥ 2157)

Topic	*Kappa*	*Response Categories (+) to (−)*	*Frequencies* Par. %	Stu. %	*Category-Specific Kappas*[a]	*Disagreement* % Dyads (stu > par)	(N)
Sociodemographic characteristics							
Student grade	.98	12	26	26	.98	48	(97)
		11	26	26	.96		
		10	32	32	.96		
		9	16	16	.97		
Student sex	.98	Female	52	52		48	(42)
		Male	48	48			
Parent birthplace	.89	N.Y.S.	74	75	.95	60	(199)
		Southern state	3	3	.80		
		Other states	12	11	.83		
		Outside U.S.	11	11	.89		
Parent employment status	.87	Working full time	58	59	.86	59	(380)
		Working part time	13	13	.42		
		Retired	1	1	.68		
		Unemployed	28	27	.88		
Parent education	.72	Grad or prof'l	10	7	.55	53	(1,302)
		College grad	8	14	.54		
		Some college	15	12	.55		
		Technical bus.	11	7	.36		
		H.S. grad	36	43	.61		
		Some H.S.	18	14	.59		
		Grade sch'l or less	3	3	.52		
Marital status	.70	Married	90	86	.77	n.a.	
		Divorced	3	6	.73		
		Separated	3	3	.52		
		Widowed	4	5	.52		

Appendix A. (Continued)

Topic	*Kappa*	*Response Categories (+) to (−)*	*Frequencies*		*Category-Specific Kappas*[a]	*Disagreement*	
			Par. %	*Stu. %*		*% Dyads (stu > par)*	*(N)*
Family income	.48	$30,000+	6	6	.48	44	(2,027)
		$20,000–$29,999	15	15	.35		
		$15,000–$19,999	22	21	.21		
		$10,500–$14,499	30	25	.24		
		$ 8,000–$10,499	15	17	.21		
		$ 5,500–$ 7,999	7	8	.22		
		$ 3,000–$ 5,499	4	6	.28		
		Under $3,000	1	2	.13		
Individual behavior: nondrug							
Frequency of student church attendance	.68	Every day	—[c]	1	.08	44	(668)
		1–2 times weekly	35	34	.64		
		1–3 times monthly	23	17	.31		
		Rarely/never	42	48	.73		
Time student spends watching TV	.35	4 + hrs. daily	4	16	.17	61	(1,339)
		3 hrs. daily	14	17	.13		
		2 hrs. daily	26	19	.08		
		1–1½ hrs. daily	36	25	.14		
		½ hour daily	12	12	.19		
		None or about none	8	11	.36		
Individual behavior: drug-related							
Parent drug use							
Frequency of:							
Smoking	.81	A pack daily or more	24	22	.69	52	(982)
		Less than 1 pack daily	12	14	.44		
		Occasionally	7	7	.40		
		Used to	24	24	.72		
		Never	33	33	.81		

Beer/wine	.54	3 + daily	1	3	.26	55	(2,189)
		1–2 drinks daily	6	7	.30		
		Several times a wk.	14	12	.29		
		About once a wk.	14	14	.21		
		1–3 times a mo.	16	16	.15		
		< Once a mo.	28	25	.24		
		Never	21	23	.49		
Hard liquor	.50	3 + drinks daily	1	1	.19	45	(2,068)
		1–2 drinks daily	4	5	.41		
		Several times a wk.	8	7	.24		
		About once a wk.	13	10	.22		
		1–3 times a mo.	17	18	.17		
		< Once a mo.	35	29	.23		
		Never	22	30	.45		
Ever use of:							
Tranquilizers	.44	Yes[b]	32	29		43	(784)
Amphetamines	.37	Yes	15	20		64	(637)
Barbiturates	.30	Yes	15	20		65	(674)
Marihuana	.21	Yes	3	4		58	(185)
Current use (in past 12 mos.) of:							
Tranquilizers	.37	Every day	1	1	.22	47	(1,011)
		Several times a wk.	1	1	.09		
		Several times a mo.	2	2	.14		
		About once a mo.	2	2	.02		
		A few times	15	15	.20		
		Not at all	79	79	.40		
Barbiturates	.24	Every day	—[c]	—[c]	−.00	73	(711)
		Several times a wk.	—[c]	1	.11		
		Several times a mo.	1	1	.08		
		About once a mo.	1	1	−.00		
		A few times	6	13	.15		
		Not at all	92	84	.26		

Appendix A. (Continued)

Topic	Kappa	Response Categories (+) to (−)	Frequencies Par. %	Frequencies Stu. %	Category-Specific Kappas[a]	Disagreement % Dyads (stu > par)	Disagreement (N)
Amphetamines	.21	Every day	1	1	.13	70	(592)
		Several times a wk.	—[a]	1	.14		
		Several times a mo.	—[a]	1	−.00		
		About once a mo.	—[a]	1	.06		
		A few times	5	9	.09		
		Not at all	93	87	.23		
Student drug use							
Ever use of:							
Marihuana	.28	Yes	20	64		98	(747)
Hard liquor	.16	Yes	6	27		95	(1,758)
LSD	.13	Yes	1	10		93	(198)
Hashish	.10	Yes	2	20		98	(654)
Barbiturates	.07	Yes	1	9		96	(311)
Heroin	.07	Yes	—[c]	1		76	(51)
Amphetamines	.05	Yes	1	10		97	(367)
Tranquilizers	.04	Yes	1	6		92	(212)
Cocaine	.01	Yes	—[c]	3		89	(103)
Current marihuana use (in last 30 days)	.05	Every day	—[c]	—[c]	−.00	97	(534)
		Several times a wk.	—[c]	3	−.00		
		Once a wk.	—[c]	2	.02		
		2–3 times	—[c]	4	−.00		
		Once	—[c]	5	−.01		
		Not at all	99	85	.06		
% of students in grade who use marihuana	.25	60–100[d]	20	49		77	(2,194)
		30–59	29	27			
		0–29	51	24			

Family Interactions and Relationships: Nondrug-related							
Parent/student closeness	.23	Extremely close	19	26	.21	48	(2,424)
		Quite close	45	34	.10		
		Moderately close	31	26	.13		
		Not particularly close	4	10	.12		
		Not at all close	1	4	.07		
Degree student depends on parent for advice/guidance	.17	Not at all	2	9	.06	33	(2,422)
		A little	32	46	.12		
		Quite a bit	49	29	.08		
		Very much	14	14	.13		
		Completely	3	2	.08		
Decision making	.16	Pt tells st what to do	3	9	.10	40	(2,446)
		Pt listens to st, then decides	40	24	.16		
		Joint decision	42	40	.10		
		St listens to pt then decides	14	20	.11		
		St decides	1	7	.11		
Freq. of talk about student:							
Educational/vocational plans	.20	About every day	9	8	.15	46	(2,206)
		Once or twice a wk.	27	31	.14		
		Once or twice a mo.	55	41	.12		
		Never or hardly ever	9	20	.19		
Personal problems	.16	About every day	20	8	.13	23	(2,631)
		Once or twice a wk.	32	20	.04		
		Once or twice a mo.	32	28	.05		
		Never or hardly ever	16	44	.15		
Freq. of praise/encouragement	.17	Every day	15	12	.12	40	(2,612)
		Several times a wk.	40	35	.10		
		About once a wk.	26	26	.04		
		About once a mo.	13	13	.08		
		Less than once a mo.	6	14	.12		
Parent rules about:							
Dress	.24	Yes	63	48		25	(1,485)
Helping at home	.22	Yes	80	66		27	(1,258)
Curfew	.21	Yes	87	66		16	(1,215)

Appendix A. (Continued)

			Frequencies			Disagreement	
Topic	*Kappa*	*Response Categories (+) to (−)*	*Par. %*	*Stu. %*	*Category-Specific Kappas*[a]	*% Dyads (stu > par)*	*(N)*
TV	.20	Yes	22	12		28	(900)
Dinner with family	.20	Yes	60	37		24	(1,681)
Homework	.19	Yes	64	38		20	(1,729)
Dating	.19	Yes	32	15		22	(1,206)
Certain boys	.19	Yes	45	26		25	(1,539)
Certain girls	.19	Yes	34	17		25	(1,276)
Parental rule about:							
Smoking	.24	Yes	59	51		40	(1,503)
Drinking	.18	Yes	63	48		32	(1,649)
Drugs	.16	Yes	77	73		44	(1,234)
Frequency of talk about drugs	.15	About every day	3	2	.12	28	(2,249)
		Once or twice a wk.	19	12	.08		
		Once or twice a mo.	53	37	.11		
		Never or hardly ever	25	49	.17		
Ease of talk about drugs	.07	Very comfortable	81	46	.09	14	(2,075)
		Somewhat comfortable	14	33	.03		
		Not at all	5	21	.05		
Individual attitudes: nondrug							
Parental educational aspirations for student	.44	Grad/profl school	23	19	.34	46	(1,610)
		College grad	49	56	.33		
		Some college	10	9	.20		
		Tech., nursing, business school	14	9	.37		
		H.S. grad	3	7	.39		
		Do not care if st. finishes H.S.	—[c]	—[c]	−.00		

Parent attitude toward student use of:							
Marihuana	.19	Tolerant	—[c]	1	−.00	70	(460)
		Tolerant if trying only	1	2	.13		
		Prefer not use	6	10	.14		
		Forbid use	93	87	.22		
Alcohol	.16	Tolerant	—[c]	5	.06	62	(,1514)
		Tolerant if trying only	11	9	.08		
		Prefer not use	25	33	.14		
		Forbid use	64	53	.20		
Mother's attitude ($N \geqslant 1{,}330$)							
Re: harm of casual use of:							
Marihuana	.20	It causes harm	64	84		19	(428)
		It doesn't cause harm	36	16			
Alcohol	.16	It causes harm	22	42		23	(608)
		It doesn't cause harm	78	58			
Student reasons for use of marihuana							
Relieve tension	.04	Yes	13	11		44	(816)
Deepen self-understanding	.04	Yes	3	5		60	(310)
Enjoy effects with friends	.01	Yes	30	13		24	(1,391)
Get pleasure	.00	Yes	38	6		10	(1,590)
Go along with friends	.00	Yes	79	5		1	(3,037)
For fun	.00	Yes	52	10		9	(2,059)
Forget troubles	.00	Yes	28	6		15	(1,216)
Rebel against parents	.00	Yes	36	2		4	(1,464)
Overcome depression	−.01	Yes	14	7		32	(773)

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[a] For variables with more than two categories.

[b] For two-category variables only one category is presented: the other ("No") contains the remaining percentage of cases.

[c] Less than .5%.

[d] An open-ended question collapsed to these categories for reporting.

The Polls:
U.S. Military Intervention

JOHN M. BENSON

IN LIGHT of recent events in Lebanon, El Salvador, and the Falklands, not to mention Poland, Afghanistan, and Iran, it seems appropriate to take a look at American attitudes toward military intervention. Senator John Tower, Chairman of the Senate Armed Services Committee, remarked at the commencement exercises at the U.S. Military Academy last May that the American people are traditionally antimilitaristic (New York *Times*, May 27, 1981), yet the United States has been involved in a number of wars and other types of military interventions since World War II. A look at some long-term trends in attitudes toward the use of American troops should shed some light on this seeming ambiguity of American foreign relations.

It is clear that since World War II the American public has not been isolationist. When asked if it would be best for the country to take an active part in world affairs or to stay out, Americans have consistently favored taking an active part (Table 1). But in the minds of many Americans, taking an active part in world affairs does not necessarily mean using American troops abroad. In February 1979, CBS News/New York *Times* asked the question in general terms: "Aside from an attack on the United States, is there any other situation when you might approve sending American troops to fight overseas?" Of the respondents offering an opinion, 69 percent said there was no other such situation.

When asked about more specific hypothetical situations that might justify the use of American troops, the American public tends to reject such an option. The Roper Organization has asked a number of such hypothetical questions (Table 2) and in most instances respondents oppose the use of troops, the most prominent exceptions being if Soviet troops invaded Western Europe and if the Soviet Union attacked West Berlin.[1] Furthermore, even in

---

[1] Even in these cases, respondents did not always favor the use of troops. Respondents in February 1981 did favor the use of troops if another U.S. embassy were taken over, but the reaction to that hypothetical case was surely conditioned by the previous crisis in Iran.

John M. Benson is a Graduate Assistant at the Roper Center for Public Opinion Research and a doctoral student in history at The University of Connecticut. He wishes to express his appreciation to Marilyn Potter, Lois Timms, Sherryl Post, and Helen Huff of the Roper Center for Public Opinion Research. Acknowledgment is also given to the survey organizations whose data were used.

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(Table 1) GALLUP, NORC, HARRIS: *Do you think it would be best for the future of this country if we take an active part in world affairs, or if we (stay/stayed) out of world affairs*[a]

	Active Part	*Stay Out*
October 1945	79%	21%
August 1947	71	29
January 1950	74	26
March 1955	77	23
June 1965	83	17
Spring 1973	68	32
Spring 1975	63	37
Spring 1976	66	34
Spring 1978	67	33
November 1980	66	34
Spring 1982	64	36

[a] Surveys by the Gallup Organization, October 1945 and August 1947; National Opinion Research Center, January 1950 through June 1965; National Opinion Research Center, General Social Surveys, Spring 1973 through Spring 1978; Louis Harris and Associates, November 1980; National Opinion Research Center, General Social Surveys, Spring 1982.

(Table 2) ROPER: *There has been some discussion about the circumstances that might justify using U.S. troops in other parts of the world. I'd like to ask your opinion about several different situations . . .*[a]

	Favor	*Oppose*	*DK*
If U.S. Embassy employees were taken hostage again in some other country			
February 1981	64%	24%	12%
If Soviet troops invaded Western Europe			
July 1978	43	43	14
February 1980	60	27	13
February 1981	51	35	14
If the Soviet Union invaded West Berlin			
July 1978	40	47	13
February 1980	54	31	15
February 1981	46	41	13
If the Arabs cut off oil shipments to the U.S. and we could obtain oil only by taking over Arab oil fields			
July 1978	30	54	16
February 1980	39	45	16
If Arab forces invaded Israel			
July 1978	21	65	15
February 1980	35	47	18
February 1981	26	58	16
If North Korea invaded South Korea			
July 1978	19	69	13
February 1981	20	63	17
If Soviet troops invaded Poland			
February 1981	23	58	19

[a] Surveys by the Roper Organization (Roper Report 81-3), latest that of February 14–28, 1981.

some less hypothetical situations the public often opposes the use of American forces. In January 1980, when CBS News/New York *Times* asked what the United States should do about the Soviet invasion of Afghanistan, only 17 percent of those offering opinions favored use of military force. When asked in March 1981 about what types of aid the United States should provide the government of El Salvador, only 3 percent of informed respondents in a Gallup survey responded "U.S. troops." In a recent example of public disapproval of the use of troops overseas, respondents to two Harris surveys of late June and July 1982 disapproved (57–43 percent and 58–42 percent) of sending U.S. Marines to Lebanon as part of the original three-nation peacekeeping force. Nor is the public's reluctance to commit troops abroad anything new. Well into the summer of 1941, Americans who were asked by Gallup how they would vote on the question of entering the war against Germany and Italy overwhelmingly opted to stay out, by 79 percent to 21 percent.

Yet is is clear that general anti-interventionist sentiment as reflected in responses to hypothetical questions has not prevented the United States from intervening frequently in foreign crises. Part of the reason for this anomaly is that anti-interventionist sentiment is far from uniform among demographic groups. The lower the socioeconomic status of the respondent (best determined by income, education, and occupation) the less likely that person is to favor use of American troops abroad (Table 3). In addition, males are more likely than females and whites more likely than blacks to opt for the use of troops. These demographic differences may very well have a considerable influence on the direction of American policy if a hypothetical situation becomes a real crisis.

The makers of American foreign policy are not a cross-section of the public. The foreign policy establishment is overwhelmingly male, white, well-educated, executive-professional in occupation, and well above average in income. Each of these demographic groups is, on the average, more likely to favor military intervention in any hypothetical situation. Foreign policy influentials from outside the government (in business, in the media) also tend to come from these potentially pro-interventionist groups. Furthermore, people of lower socioeconomic status, as well as women and blacks, tend to be much less certain about their position, an uncertainty reflected in the higher numbers of "Don't know" responses. In any foreign policy crisis, therefore, groups that are potentially more favorable to intervention have considerably more influence over policy than those that are potentially less favorable.

This does not mean, however, that the makers of foreign policy are necessarily taking a great risk in public opinion by intervening militarily in a crisis. In fact, public response to active American military interventions has been initially supportive. After President Eisenhower sent the Marines into Lebanon in 1958, Gallup International Research Institutes asked respondents in several cities around the world if they approved or disapproved. Respondents in New York, Chicago, and San Francisco approved 59 percent to 27 percent. In a Gallup Poll taken shortly after President Johnson sent troops to the Dominican Republic in late April 1965, respondents approved of the decision 76 percent to 17 percent. Shortly after the major escalation of the Vietnam War in the summer of 1965, the Gallup Organization found that only 24 percent felt that the United States had made a mistake in sending troops to

(Table 3) **ROPER:** *There has been some discussion about the circumstances that might justify using U.S. troops in other parts of the world. I'd like to ask your opinion about several different situations. . .*[a]

	If Soviets Invaded W. Berlin			*If Arabs Invaded Israel*			*If Soviets Invaded W. Europe*			*If Soviets Invaded Poland*			*If N. Korea Invaded S. Korea*			*If Cubans Involved in Communist Takeover of Central Amer. Country*			*If U.S. Embassy Employees Taken Hostage Again in Country*		
	Fav. 46%	*Opp.* 41%	*DK/NA* 13%	*Fav.* 26%	*Opp.* 58%	*DK/NA* 16%	*Fav.* 51%	*Opp.* 35%	*DK/NA* 14%	*Fav.* 23%	*Opp.* 58%	*DK/NA* 19%	*Fav.* 20%	*Opp.* 63%	*DK/NA* 17%	*Fav.* 42%	*Opp.* 42%	*DK/NA* 16%	*Fav.* 64%	*Opp.* 24%	*DK/NA* 12%
Sex																					
Male	55	36	9	30	57	12	61	30	9	25	59	16	28	60	13	50	38	12	71	20	9
Female	38	45	16	22	59	19	42	40	18	22	57	22	13	67	20	34	46	20	57	28	15
Household income																					
Under $10,000	35	49	16	20	60	19	36	44	21	17	60	23	15	65	20	34	48	19	52	33	15
$10,000–under $20,000	44	42	14	26	57	17	50	34	16	22	57	21	21	62	17	41	43	16	63	22	15
$20,000–under $30,000	52	37	11	28	57	16	57	33	10	26	57	17	24	61	16	46	40	15	69	21	10
$30,000 and over	58	34	8	31	60	9	64	29	7	29	59	12	21	68	12	47	38	15	72	21	7
Race																					
White	51	37	12	28	56	16	56	31	13	26	55	19	22	62	17	44	40	16	66	22	12
Black	15	69	16	12	73	15	17	67	16	8	75	17	10	74	16	23	60	18	44	37	19
Education																					
Non HS grad	36	48	15	12	16	20	36	45	19	19	59	22	18	63	20	35	47	18	55	31	15
HS grad	45	42	14	57	60	58	48	38	14	27	55	18	18	65	17	44	40	15	66	21	13
College	54	36	10	30	24	22	64	26	10	23	59	18	23	62	14	44	41	16	67	22	11
Occupation																					
Blue collar	50	41	9	29	57	15	54	35	11	28	55	17	24	61	16	50	38	12	68	22	10
White collar	49	37	14	26	60	14	56	32	12	24	60	16	20	66	13	37	46	17	62	27	11
Exec., prof.	58	33	8	35	56	9	69	24	7	28	58	13	26	62	12	47	39	14	76	16	8
Political affiliation																					
Democrat	42	46	12	25	62	13	45	40	15	22	60	18	19	66	15	38	47	16	60	27	13
Republican	56	34	10	29	53	18	62	28	10	27	57	16	24	59	16	51	35	15	71	20	10
Independent	45	40	15	25	57	18	50	35	15	22	56	22	18	63	18	41	41	17	66	22	12
Political philosophy																					
Conservative	53	35	12	28	56	16	57	30	13	26	57	17	25	60	15	49	37	15	68	22	11
Moderate	44	42	14	25	58	17	49	36	15	22	56	22	16	65	19	40	42	18	63	23	14
Liberal	42	49	9	25	63	12	48	41	11	23	63	15	17	70	13	35	53	13	61	29	10
Age																					
18–29	43	46	12	29	55	16	53	35	11	25	58	17	21	65	14	42	44	14	68	23	9
30–44	51	36	13	27	59	14	54	31	14	26	55	19	22	60	18	43	41	16	69	20	12
45–59	49	41	10	25	61	14	51	36	12	23	61	17	20	66	14	43	41	16	63	26	12
60+	42	40	18	21	58	20	44	38	19	19	57	24	17	61	22	38	43	20	52	29	19

[a] Survey by The Roper Organization (Roper Report 81-3), February 14–28, 1981.

Vietnam, while 61 percent said it was not a mistake. And when *Time*/Yankelovich, Skelly, and White asked if charges that President Ford overreacted to the seizure of the *Mayaguez* in order to re-establish American prestige were fair or unfair, 87 percent of respondents who offered an opinion said the charges were unfair.

Not only have military interventions generally been initially popular with the American public, but presidents have benefited from the interventions politically: President Roosevelt's popularity rating jumped 12 percent in the wake of his declaration of war on Japan following Pearl Harbor; President Kennedy's ratings also jumped 12 percent after the Cuban missile blockade; President Johnson's rating went up 8 percent after the bombing of Hanoi in 1967; and President Ford's rating climbed 11 percent after the *Mayaguez* incident.[2] However, when the Iranians first took American Embassy personnel hostage, President Carter's rating as measured by the Gallup Organization jumped 13 percent, even though the United States did not intervene militarily at that time. The rise in presidential ratings, therefore, seems to reflect a rallying around the leader during a crisis rather than an approval of a specific policy (cf. John E. Mueller, *War, Presidents and Public Opinion*, N.Y.: Wiley, 1973, pp. 208–13).

The jump in presidential approval in times of crisis takes place across all demographic groups, with the rise somewhat more pronounced in the higher socioeconomic groups than in the lower.[3] In any crisis that is not resolved quickly and successfully by military intervention, however, pro-intervention and anti-intervention groups tend to react differently from each other. In December 1979, three weeks after the Iranians had taken the U.S. embassy in Tehran, the Roper Organization asked whether the Shah (who had been admitted to the U.S. for medical treatment) should be returned as the Iranians demanded (Table 4). The lowest socioeconomic groups and blacks gave their approval to this option considerably more often than the higher socioeconomic groups and whites. The anti-intervention groups, however, were also slightly more likely to favor another extreme position, namely, bombing Iran. (See Mueller, pp. 122–36, on similar patterns in response to the Korean and Vietnam wars.) Again, men favor the more militaristic positions more often than women.

Furthermore, a prolonged crisis or war can change the initial strong approval of intervention into disapproval, as the cases of Korea and Vietnam demonstrate. For instance, the percentage of respondents in Gallup surveys to the question of whether or not the U.S. made a mistake in sending troops to Vietnam went up from 24 percent in August 1965 to 53 percent in August 1968 to 61 percent in May 1971.

The question of whether the American public is anti-militaristic is therefore difficult to resolve. Even though public opinion since World War II has not been isolationist, the polls show that when hypothetical situations are proposed, people are reluctant to intervene militarily. When an actual crisis occurs, the public tends to rally around the president in the short run but becomes less supportive if a crisis or war drags on with no resolution in sight. There are also demographic differences in the response to both hypothetical and actual crises.

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[2] *Gallup Opinion Index*, Nov. 1978, page 3.

[3] The approval rating among men changes more than among women. Men seem consistently to favor more militaristic solutions to problems than women.

(Table 4) ROPER: *Now that the Shah is here in this country, the Ayatollah Khomeini says we must return him to Iran for trial or else they will not return our hostages. Some people say we should return the Shah to Iran for trial in order to save the American hostages; others say we cannot yield to such a demand, even if it risks the hostages' lives. Do you think we* should, *or should* not *return the Shah to Iran?*

Various people have proposed a number of different approaches to dealing with the hostage situation in Iran. Bearing in mind the effect each of them might have on the hostages, on the reactions of other Arab countries, on possible responses by the Russians and on world reaction in general, would you tell me for each one whether we should or should not do it?[a]

	Return Shah				*Bomb Iran*		
	Should 23%	*Should Not* 56%	*Send Out but Not to Iran* 13%	*DK* 7%	*Should* 9%	*Should Not* 86%	*DK* 5%
Sex							
Male	20	63	12	5	12	84	5
Female	26	51	13	10	7	88	6
Household income							
Under $10,000	32	42	13	14	10	81	9
$10,000–$19,999	27	51	13	9	10	86	5
$20,000–$29,999	20	61	13	5	8	88	5
$30,000 and over	16	70	11	4	8	88	4
Race							
White	21	60	13	7	9	86	6
Black	42	12	12	13	11	85	4
Education							
Non HS grad	36	39	14	11	11	81	8
HS Grad	23	58	12	7	10	86	4
College	14	69	12	6	6	89	5
Occupation							
Blue collar	30	49	14	7	11	85	4
White collar	20	65	10	6	9	85	6
Exec., Prof.	11	70	11	7	7	92	2
Party							
Democrat	26	53	12	9	10	83	6
Republican	18	67	10	5	7	90	3
Independent	20	57	16	7	9	86	5
Political philosophy							
Conservative	23	59	11	7	10	86	4
Moderate	22	56	15	7	8	86	6
Liberal	26	55	13	7	9	87	5
Age							
18–29	28	56	9	7	11	83	6
30–44	21	59	14	6	9	86	5
45–59	21	59	13	7	8	89	3
60+	23	51	15	11	7	86	7

[a] Survey by the Roper Organization (Roper Report 80-1), December 1–8, 1979.

Responses to questions by NORC concerning military spending perhaps sum up the American attitude on intervention (Table 5). Since the end of the Vietnam war, with only one exception, a plurality of Americans has felt that military spending is about right. In the immediate aftermath of the unsuc-

(Table 5) NORC: *We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount . . . Military, armaments, defense.*[a]

	Too Little	*About Right*	*Too much*
1973	12%	48%	40%
1974	18	49	33
1975	18	49	33
1976	26	45	29
1977	26	49	25
1978	29	47	24
1980	60	28	12
1982	31	38	32

NOTE: 1980 Survey conducted in midst of Iranian crisis.

[a] Surveys by the National Opinion Research Center, General Social Surveys, latest that of February–April, 1982.

cessful Vietnam war, many more people felt that the U.S. was spending too much than thought we were spending too little on the military. The only exception to this apparent anti-military or moderate pattern occurred in 1980, when the NORC survey was conducted in the midst of the Iranian crisis.

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The Methodology and Performance of Election Day Polls

MARK R. LEVY

ON ELECTION day 1980, more than 83 million Americans cast ballots in the presidential election. Some 36 thousand of them "voted" twice—the second time as respondents in three different election day surveys conducted in voting districts nationwide by five major news organizations. Known as election day, or exit, polling, this survey technique has become the principal method by which the national news media assess the characteristics, attitudes, and electoral decisions of the American voter.[1]

The first known exit poll by the major media was carried out in 1967, when members of what was to become known later as the CBS News Election and Survey Unit experimented with the method in three off-off-year elections. Prior to election day polling, journalists

[1] Information about the origins of election day polling, as well as the methodological details presented below and certain data, was collected through personal interviews and/or correspondence carried out during the summer and fall of 1981 with the following: Linda Cranney, ABC News Political Unit; Fred Soulas and Dan Hagen, Chilton Research Services; Warren Mitofsky and Kathleen Frankovic, CBS News Election and Survey Unit; I. A. Lewis, *The Los Angeles Times* Poll; Roy Wetzel and Les Crystal, NBC News; Steve Teichner, Teichner Associates, Inc.

Abstract Methodological details are presented of a survey research technique known as exit or election day polling. Using this technique, major American news organizations collect and analyze voting and attitude data from samples of persons who have just cast ballots. On the basis of the 1980 elections, differences in polling strategies and performance of the exit poll method are examined. How election day survey data are used by journalists is discussed.

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and, to a lesser degree, academic students of the election process, had relied primarily on preelection surveys and vote returns from selected precincts to interpret the vote (Bicker, 1978; Bohn, 1980). But many election analysts, within and outside the media, believed those two methods to be seriously flawed. Preelection surveys, for instance, suffered from the classic "screening" problem of how to identify likely voters (Crespi, 1977), and more important, because of publication deadlines, they often required early field work closeout and were thus unlikely to capture late shifts in voter sentiment.

Precinct-level vote data too were recognized as having substantial journalistic and scientific limitations. First, election returns contained no explicit information about voter attitudes and perceptions, although journalists often inferred them anyway. Second, so-called tag or analytical precincts were often selected through purposive sampling of election districts with disproportionate concentrations of voters having shared demographic characteristics (race, religion, income, etc.) or with strong partisan voting histories. See, for example, Levy and Kramer (1972).

Aware of the ecological fallacy (Robinson, 1950), concerned that "ghettoized" voters did not represent all voters sharing a given demographic attribute, and faced with increasing difficulty in locating homogeneous precincts, media election analysts turned to election day polling. By 1980, the three television networks, the Associated Press, *The New York Times,* and *The Los Angeles Times* were all surveying voters as they left the polls.[2]

This article outlines the methodology of election day polling and compares the different approaches taken in four major polls.[3] Examples and data are drawn from the 1980 presidential primaries in New Hampshire, Florida, and California, and from the November 4 general election. This report focuses exclusively on the presidential campaign, although election day polls using virtually the same methods have been and continue to be routinely conducted in state and local contests as well.

The elections selected for analysis here were chosen because the author believed that these contests represent the range of challenges faced by the profession. New Hampshire, for instance, is a small, relatively homogeneous state which traditionally holds the first presidential primary. As such, it is considered an important first test, both for political hopefuls and for the journalists who report on them (Matthews, 1978). Florida, the first major Southern primary, is an instance of election day polling under conditions of potential adversity, since state election law prohibits exit interviews within 100 yards of any polling place.[4] The California primary comes late in the political season and is held in the nation's most populous and diverse state. The November election requires large-scale surveying nationwide all in one day, confronts pollsters with enormous logistical problems, and is marked by especially intense competition between news organizations.

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[2] Although six news organizations were involved in election day polling, there were actually only three national election day surveys in November 1980, since CBS News conducted its polls in association with *The New York Times,* and NBC News teamed up with AP. The L.A. *Times* polled only during the presidential primaries.

[3] In addition to these four election day polls, Teichner Associates, Inc. of Princeton, N.J. has conducted approximately 20 exit polls since 1979 for broadcast clients interested in local and statewide elections in major television markets nationwide.

Like all survey research, election day polling is an amalgam of elements: sample design, questionnaire construction, field work, and data collection-analysis. I will now consider each in turn.

Sample Design

Exit interviews in the 1980 elections were conducted in precincts or election districts sampled at random from frames which usually included all precincts in a given primary state or in the case of the general election, all voting districts nationwide. In both types of elections, precincts were usually grouped into two to six strata, the basis of such dimensions as past voting behavior, geographic regions (within or across states), urban vs. rural counties, percent foreign stock, type of voting equipment, or poll closing times.

Since most precincts nationwide do not contain equal numbers of registered voters, sampling within strata was usually conducted with probabilities proportionate to either the total number of votes cast within the precincts in some "base year" or proportionate to current voter registration. In the CBS News/New York Times poll, for example, the sampling frame was a general election from either 1976 or 1978, and that frame was used to determine sample points for both 1980 primaries and the November election. *The Los Angeles Times*, which carried out only primary day polls, based its precinct selections on proportionate sampling tied to 1980 voter registration figures. By contrast, the ABC News survey generally developed its primary state sampling frames from 1976 presidential primary votes, with selection of precincts usually conducted using a PPS technique. In a few exceptions where precincts had roughly equal numbers of voters, ABC used a stratified random sampling technique in which precincts had an equal probability of selection.

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[4] Florida election law 104.36 reads in part: "Any person who, within 100 yards of any polling place on the day of the election, . . . solicits or attempts to solicit any vote, *opinion*, or contribution *for any purpose* . . . is guilty of a misdemeanor of the first degree. . . [emphasis added].

The selection of a base year is, of course, something of an art, requiring judgments about the comparability of past and current political conditions, as well as an elaborate staff mechanism for collecting vote-registration data, information about redistricting, and other items of political intelligence. Exit pollsters were constantly aware that turnout patterns in 1980 might deviate in some unpredictable fashion from those in the base year, thus causing them to draw samples which over- or underestimated geographic regions, demographic groups, and the like. As a result, base year data were occasionally manipulated to reflect changed political conditions.

At CBS News, election day poll precincts were actually a random subsample of a larger precinct sample, drawn to provide actual vote data for the network's estimation or projection model, which attempted to predict vote outcome before the total vote had been tallied. The L.A. *Times* devoted its entire precinct sample to obtaining data for analytical purposes, although vote returns were also collected from sample precincts to feed a computer projection model whose output was used primarily to improve exit poll results for analytical purposes. By contrast, both NBC News and ABC News drew separate precinct samples for projection and analysis purposes.

Of all the polling organizations, only NBC News drew separate precinct samples for the Democratic and Republican primaries. Using the same sample points for both party primaries was in all cases a decision based on limited budgetary and logistical resources.

The one exception to the general rule of sampling based on previous vote returns or current registration figures was a procedure employed by ABC News in the November election. For its general election poll, ABC used a multistage probability design coupled with telephone interviewing to identify voting households and polling places.

Across news organizations, the number of precincts chosen as exit poll interview sites varied from 23 to 75 in the primary states and, in the general election, from 150 (CBS), to 375 (ABC), to 400 (NBC).[5] CBS News consistently sampled the fewest number of precincts, sometimes only one-third the number of interview points used by NBC, which always had the largest number.[6] (See Table 1.)

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[5] Sample *N*'s for the November general election were: ABC News—9,341, CBS News-New York Times—12,782, and NBC News—11,629.

[6] In order to supply more detailed reports for their owned-and-operated stations and because of certain hotly contested statewide contests, CBS News oversampled New York state and California, and NBC News said it expanded its samples in 10 states for the general election.

Table 1. Selected Exit Poll Sample Design Characteristics

	New Hampshire Primary				Florida Primary				California Primary			
	ABC	*CBS*	*NBC*	*L.A. Times*	*ABC*	*CBS*	*NBC*	*L.A. Times*	*ABC*	*CBS*	*NBC*	*L.A. Times*
Number of sample precincts												
Dem. primary	45	25	40	51	60	23	75	51	70	28	75	51
Rep. primary	45	25	60	51	60	23	75	51	70	28	73	51
Sample *N*												
Dem. primary	646	687	1,194	991	1,089	922	2,050	1,238	1,179	1,131	2,128	1,780
Rep. primary	920	967	1,760	1,273	660	587	2,003	805	1,194	801	1,930	1,321

For all exit pollsters, the number of precincts sampled represented a compromise between budget limitations and sampling theory, since with fewer precincts and more interviews per precinct, the number of independent observations of course declined and sampling error increased. On the other hand, more sample points required more interviewers, raising survey costs. CBS News justified its smaller sample point design on the basis of proprietary studies which reportedly demonstrated that of all possible sources of error, interviewer effects, stemming particularly from faulty respondent selection, and not the number of sample points presented the greatest threat.

Within precincts, all election day polls selected respondents on a systematic random basis. Interviewers were generally provided with a skip interval and told to approach every *k*th voter, counted after an initial starting time. Skip intervals were tied to expected voter turnout and the total number of interviews desired, and were set on a precinct-by-precinct basis. Exit pollsters attempted to set their skip intervals in such a way as to give interviewers a case load which kept them neither too busy nor too bored and which would insure rigorous compliance with the selection protocal.

The actual skip intervals used ranged from "every voter" in unusually small precincts to one in 70 or more in the largest. Skip intervals were generally in single digits for primaries (where turnout was lower); while in the general election, intervals were typically one in 10 or one in 20. Skip patterns were usually held constant during the voting day, regardless of turnout. The one exception was in the ABC News poll, where the skip interval was occasionally modified to reflect turnout and to maintain roughly equal numbers of interviews from each sample point. This practice, which was followed in fewer than 10 percent of all cases, may have introduced a chronological bias of unknown magnitude into some samples, since it has been demonstrated (Klorman, 1976) that voters with different socioeconomic characteristics vote at different times of day. Thus, by adjusting the skip interval, ABC News ran the risk of unwittingly affecting the representativeness of part of its sample.

Questionnaires

All election day polls employed a "secret," *self-administered* questionnaire. Respondents were asked to answer between 20 and 50 items, generally printed on two sides of a form, letter-sized or smaller. Included among the questions were a self-report of vote(s) just cast; demographic measures such as sex, age, education, occupation, income; measures of political ideology and partisanship; time of and influences on vote decision; and attitudes toward selected candidates and issues. All questionnaire items were closed-ended. Scales of ideology and partisanship typically were truncated to three steps, while demographic measures were always collapsed into moderately broad ranges of possible responses. Completed questionnaires were folded by respondents to obscure answers and deposited in a "ballot box" carried by interviewers.

My analysis of CBS News data showed that from 5 to 10 percent of all respondents generally did not answer any items printed on the reverse side of the "ballot"—a loss of upwards of 20 items, including such key demographic variables as respondent education, family income, and religion. The same analysis found that older voters, women, and blacks were significantly more likely to leave the reverse side of the questionnaire incomplete.

Field Work

Election day interviews were conducted either by professional survey research interviewers or by college students and other "irregulars." Field staffs were recruited either directly by state supervisors working for the polling organizations or through subcontracts with other field work organizations. Field work costs, including interviewer fees and supervision, were estimated by industry sources to average between five and ten dollars per completed interview.

Of all the exit polls, only ABC's instructed interviewers to arrive in the designated precincts exactly at poll opening time and to attempt interviews throughout the entire voting day. CBS News generally withdrew its field force an hour or so before the polls closed, believing that few votes would be cast in the final hour. *The Los Angeles Times* staffed its precincts on a rotating basis throughout the day, with six different patterns of coverage employed at random; while NBC News also used a rotation scheme which limited the time for interviewing.

After arriving at the polling place, interviewers were asked to identify themselves to election officials, to determine the number of physical exits from the building housing the voting equipment, and to locate a telephone which would be used three or four times during the day to call in partial survey results as well as the actual precinct vote.[7] In general, one interviewer was assigned to each precinct, although that number was occasionally doubled in locations with large expected turnout or in "bad" neighborhoods.

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[7] Warren Mitofsky of CBS estimates that in his poll it requires about two hours of telephone time to gather poll and vote data from one sample precinct.

Exit poll interviewers were instructed to stand as close to the voting site as local election officials and laws permitted, and to move between all exits in a pattern roughly proportionate to the amount of voter traffic leaving from each. Once a potential respondent had been identified by means of the skip interval, interviewers, except those working for ABC News, were instructed to record an estimate of that respondent's sex, age, and race on the questionnaire, and to hand it to the potential respondent. If a voter refused to fill out the questionnaire, CBS News and the L.A. *Times* interviewers were expected to attempt to interview the next *k*th voter in keeping with the skip interval. In the ABC and NBC polls, however, interviewers were instructed to approach the very next voter, and if that person refused, to start their skip-count again, attempting to reach, in effect, the next $k+l$th respondent.

Industry studies suggest that election day polls have a completion rate roughly comparable to household surveys of the general population (75–90 percent), although lower rates were not infrequent; and that women, blacks, and older voters were somewhat less willing to be interviewed. Some exit pollsters believe that refusal rates are roughly a function of questionnaire length, and they attempt to disguise the number of questions asked by shrinking the size of the "secret ballot." However, since most refusals reportedly occurred as respondents were approached by interviewers—and thus before a potential respondent had read the questionnaire—other pollsters disagree with that analysis.

In addition to the problem of refusers, all election day polls faced a potentially serious problem in those locations where several precincts voted in the same building, since interviews were supposed to be conducted only with voters from the precinct designated in the sample design. However, in multiprecinct locations there was no way for interviewers to know in which precinct a respondent had just voted. In some states up to one-third or more of all survey sites were of this multiprecinct type. Typically, election day pollsters dealt with this problem by assuming that voters in multiprecinct locations were roughly homogeneous and by manipulating the skip interval by a factor equal to the number of precincts present.

Both NBC News and the L.A. *Times* surveys set a quota of interviews to be completed by the end of each interview period or wave, with unmet quotas to be filled in subsequent periods. All other surveys relied on the skip interval to generate adequate numbers of responses. About 75 to 80 percent of interviewers' field time was spent in data collection, with the remainder devoted to telephoning data into a centralized phone-bank, meal breaks, and the like. Most polls scheduled noninterviewing to coincide with slack voting times, although the CBS News poll set aside 10 minutes each hour for noninterviewing, regardless of turnout. Immediately before the prescheduled data reporting times, interviewers were asked to determine from election officials the actual number of votes cast up to that point.

Data Manipulation

All election day polls, with the exception of that by ABC News, applied various weighting procedures to their data. Because of its sample design and field work methods, ABC's data were essentially self-weighting. The most elaborate weighting strategy was that developed by CBS News, and it can serve as an illustrative model.

The CBS News/New York Times survey applied three different weights to its data. The first was a within-precinct weight which took into account the probability of *respondent* selection and differential noninterview rates within precincts. That weight was calculated from the following:

$$\text{Weight} = \frac{v}{n}$$

where v = total precinct vote, and n = completed interviews.

If the polls were closed, then the total precinct vote (v) would be the actual total vote cast that day as reported by election officials. If voting was still in progress, then an estimated total vote, based on historical data and current turnout figures, was used in the numerator.

Next, a precinct weight was applied to sample returns which were already weighted to the precinct level. This weight took into account the unequal probabilities of *precinct* selection and was calculated from base year election data.

While the first two weights were a direct consequence of sample design, the final weight was applied in order to decrease sampling error. This third weight was a ratio estimate in which precinct-level returns aggregated across the entire sample are manipulated in accordance with expected vote outcome. The estimate of expected vote outcome itself was derived from a computer model, driven initially by election day poll data and later in the evening by actual vote returns. That model's prime function was to provide vote estimates or projections, but those vote estimates were also available to "force" the election day survey data into accordance with expected vote outcomes.

A general formula for ratio estimated weights is:

$$E = \left(\frac{\Sigma x_i}{\Sigma y_i}\right) Y$$

where E = estimated final vote for a candidate "tonight" (e.g., Reagan vote, 1980)

x_i = weighted precinct-level vote for a candidate "today" (e.g., Reagan vote, 1980, Baltimore Ward 3, Precinct 11)

y_i = weighted, known precinct-level vote for "comparable" candidate in previous election (e.g., Ford vote, 1976, Baltimore Ward 3, Precinct 11)

Y = actual statewide vote for "comparable" candidate in previous election (e.g., Ford, 1976 general election, Maryland)

Since the ratio estimated weight provided an expected vote outcome for only one candidate at a time, the procedure was repeated for as many candidates as were running. Once the entire weighting process was completed, analysis of the survey began, using weighted data.

Using Exit Polls

News organizations used election day poll data in two ways in 1980. The first, as raw material for analytical news stories and sidebars, has become a routine and highly regarded part of election reporting; the second, as a pseudo-projection or "early reading" on the election outcome, is a matter of some controversy among polling professionals and broadcasters.

With regard to the first use, results of exit polls taken on primary days were typically included in all "feeds" and updates of the network evening news, in late evening special reports, and often in second-day follow-up stories for the networks' early morning news programs. Newspaper and wire service use generally included election day poll data about "early trends" for newspaper first-edition reports, one or more extended analytical stories for the late Wednesday editions, and occasional second-day stories which appeared in the Thursday newspapers.

In the general election, continuous network coverage began with the early evening news, and all three networks remained on the air until well after midnight. Throughout the evening, CBS News set aside specific time slots for stories based on election day poll data, while ABC News and NBC News tended to integrate their poll results into ongoing reports. Wire service and newspaper use of exit poll results followed the pattern of the primaries, with the addition of extended week-in-review reports, often with elaborate graphics, for the Sunday papers.

While the use of election day poll data as an analytical tool clearly enhanced news coverage, simply having exit poll data—and having it available before the voting was completed—led to another, highly controversial use. In most elections, between one-third and one-half of all exit poll responses were gathered and tabulated by one or two o'clock in the afternoon, with additional data scheduled to be called in before 5 or 6 P.M. As a result, election day poll data were available and often used to give shading and tone to early broadcasts and stories for first editions. Qualitative adjectives such as "strong," "surprising," "commanding," and the like were often attached to vote return figures, generally with no explicit statements regarding the tentative nature of the vote results. Since the network reports often aired prior to the close of primary voting or, in the case of the general election, before the end of the voting day in the West, it is possible that such pseudo-projections may have affected people who had not yet voted, keeping them from the ballot box or perhaps drawing them to the polls in an "underdog" or "bandwagon" effect (Lang and Lang, 1968).[8]

The issue of early projections and West Coast voting is, of course, not a new one (Miller, 1967; Jackson and McGee III, 1981). However, there has been no research which focuses specifically on the impact of exit-poll-generated pseudo-projections and voter turnout.[9] Such research is clearly beyond the scope of this article, but the relationship between election day polling and the integrity of the electoral process must nevertheless be a matter for immediate attention by both journalists and survey researchers.

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[8] As Thomas H. Wyman, President, CBS Inc., observed in a speech on October 7, 1981 to the International Radio and Television Society in New York City: "While employing the latest research techniques, such as exit polling, to inform voters, we must be careful to approach the task responsibly. We cannot permit any confusion between commentary based on exit-polling interviews and the announcement of results."

[9] While both ABC News and CBS News have specific rules which prohibit those networks from "projecting" election winners without estimates based on actual vote returns, NBC News followed a policy of "calling winners" based solely on election day poll data, if the outcome was clear and if voting was completed in a given primary state. Moreover, NBC News was the first network to project the 1980 Reagan victory, and did so with the polls still open on the West Coast, because its exit polls suggested a landslide. NBC announced Reagan victories in 22 states at 8:15 P.M. (EST), and most of these were announced within one minute of the poll closing time. These "calls" were based either on exit polls or no official vote data.

Poll Performance

Unlike most public opinion polls, preelection surveys and exit polls can, in principle, be directly validated. The Gallup Organization, for instance, reported that since 1936, its final, preelection poll has deviated from the actual returns by an average of 2.8 percent (The Gallup Organization, 1981). Even though election day surveys are not primarily intended as predictive tools, a similar standard can be applied in an attempt to assess their performance. Moreover, since exit polls ask respondents to indicate how they voted almost immediately after the voting act, one would expect that election day polls would capture an especially accurate portrait of the electorate.

Table 2 charts a comparison between election day sample estimates of vote percentages for major candidates and the actual statewide or national vote returns. The comparison is based only on data from the ABC News poll, since of all the exit polls only ABC's was designed to produce meaningful data without weighting.[10]

For my analysis, sample percentages for winning candidates were subtracted from the vote percentage for that candidate as reported by election officials. I call that value "sample deviation." As a further step, vote returns were considered as a binomial variable, with the winner's percentage of the vote making up one value, and the sum of all other major candidates' percentages a second. By taking into account the size of the election day samples, the frequency distribution of the binomial voting-percentage variable, and an adjustment for the cluster sampling design, it was possible to determine the degree to which sample deviation varied from theoretically expected sampling error.[11]

In three primaries, ABC's sample percentages for winning candidates departed from actual vote returns by an average of 3.7 percentage points, with a range of sample deviations of from 0.6 to 5.8 percentage points. In the November sample, exit poll data deviated from the final vote percentage by a miniscule 0.2 percent. For five of the seven elections examined, ABC's sample percentages were

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[10] Partially weighted data, based, e.g., on the first two weights used by CBS, were unavailable; and by definition completely weighted data would not deviate very much, if at all, from actual returns.

[11] The effect of cluster size on sample error was evaluated by the following formula: DEFF = 1 + rho $(\bar{n}-1)$, where rho is an estimate of cluster homogeneity and $\bar{n}$ is the average size of the sample from each cluster (Sudman, 1976). The value of rho used here was empirically derived by Dan Hagen of Chilton Research Services by means of a Taylor series approach to calculating variance for complex samples, and used the computer program authored by B.V. Shah which is available through the Statistical Analysis System (SAS). The Shah program was applied to data from one primary state and the national election in order to estimate rho.

Table 2. Sample Performances ABC News Election Day Polls

	Primaries						
	New Hampshire		*Florida*		*California*		*Nov. General Election*
	Dem.	*Rep.*	*Dem.*	*Rep.*	*Dem.*	*Rep.*	
Theoretical sampling error	±6.0%	±5.6%	±4.6%	±5.4%	±4.7%	±3.3%	±2.0%
Actual "sample deviation"	−4.9%	+5.8%	+2.7%	+4.2%	−0.6%	−3.8%	−0.2%

smaller than theoretical sampling error, while in two of the seven contests, "sample deviations" were only very slightly larger than expected (Table 2).

From this analysis, we see that the first ABC News poll in New Hampshire performed about as well as the final surveys in California, and that geographic size or demographic complexity did not prevent accurate polling. Additionally, Florida's restrictive law regarding exit interviews did not hamper the ABC poll.

Discussion

Election day polling stands as a *tour de force* of survey methodology, wedded to the information needs and resources of the American mass media. Every two years, thousands of journalists, analysts, interviewers, computer programmers, data clerks, and others are mobilized for a single day's effort. In that effort, election day pollsters have learned not only how to gather large amounts of survey data quickly, but how to analyze and interpret their findings in the face of enormous competitive pressures and time constraints.

The exit polls examined here appear to have been conducted with a degree of meticulousness which compares favorably to the highest standards of commercial and academic research. Indeed, academic and other researchers might profitably carry out secondary analyses of election day poll data, using the CBS/New York Times, L.A. *Times*, or ABC News polls, since all are available from public archives.

This is not to say that all election day polls were textbook perfect, for they were not. There were some problems with basic sample frames and the choice of certain base years, and the quality of field work remains a continuing source of concern. On the other hand, the comparative vastness of exit poll samples, particularly in the general election, and the virtually coincidental nature of the interviewing suggests some real strengths. Overall, election day polls are an efficient and constantly improving technique—a method for gathering data which has enormously enhanced the practice of election reporting.

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The Polls: Attitudes Toward the Arab-Israeli Conflict

CONNIE DE BOER

SINCE its inception, the state of Israel has been repeatedly at war with its Arab neighbor countries: in 1956, the Suez crisis and the Israeli occupation of the Sinai; in June 1967, the Six-Day War; in October 1973, the Yom Kippur War; and in 1982, the invasion of Lebanon by Israeli forces. In this issue of The Polls we present results of opinion surveys in the United States and in some Western European countries on sympathy for Israel and the Arab countries, on the Palestinians and the PLO, on arms supplies to the countries of the Middle East, and on recent developments in Lebanon.

The question of support for Israel or for the Arab countries was frequently submitted to the public by Gallup from 1967 through 1982. The results of these surveys show an increase in American sympathy for Israel during and immediately after the 1967 and 1973 wars and after the withdrawal of the Israeli forces from the Sinai at the end of April 1982. The percentage of those sympathizing with Israel was fairly constant during the intermediate periods.

The invasion of Lebanon on June 6, 1982 did not have the favorable effect on public opinion in the United States that earlier wars had elicited. There was hardly change in the results of opinion surveys into the sympathies expressed in favor of Israel and of the Arab countries. But some days after the massacres in the Palestinian camps of Sabra and Shatila (September 18–19, 1982), a Gallup poll conducted for *Newsweek* revealed a decline in the sympathies of the American people toward Israel: 32 percent were more sympathetic to Israel (vs. 49 percent in July 1981), and 28 percent were more sympathetic to the Arab nations (vs. 10 percent in July 1981). According to another Gallup survey, the favorable opinion on Israel appears to have declined even before the massacre in the Palestinian camps. In 1981, 75 percent of Americans had a favorable opinion of Israel, but by mid August 1982 this percentage had dropped to 56, lower than it had been during the preceding 20 years.

In the countries of Western Europe, sympathies with Israel have been declining since about 1973, although not in favor of the Arab countries; rather, a preference for neutrality is being expressed. In West Germany, however, the opinion polls conducted by Demoskopie Allensbach did reveal a marked upswing in favor of Israel after the Yom Kippur War (October 1973).

One of the major problems in the Middle East conflict is the question of what is to happen with the Palestinian people. Mediators in the conflict have not yet succeeded in working out a solution acceptable to both parties. The public, too, is finding it difficult to pronounce itself in favor of any of the possible choices offered in opinion polls. The percentage of "don't knows" often exceeds 30,

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even where the question is submitted only to the part of the sample having stated its awareness of the situation in the Middle East.

Israel does not accept the PLO, the best-known organization of the Palestinians, as a party in the discussions on finding a solution for the Palestinians, many of whom are now scattered through several countries, and the PLO refuses to explicitly recognize Israel's right to exist. A Newsweek/Gallup survey conducted in early August 1982 indicates that 48 percent of Americans feel that the United States should talk directly with the PLO as the representative of the Palestinian people, even though the image of the PLO as a terrorist organization has remained intact. In Great Britain, the percentage of the population willing to include the PLO in peace negotiations rose from 49 in 1976 (NOP) to 61 in August 1982 (MORI).

Arms supplies by the United States to Israel are approved in the polls by a large majority of the public only where the question also mentions Russian arms supplies to Arab countries (Harris, 1975). Furthermore, Gallup research indicates that 64 percent of Americans feel that the United States should require that all weapons sent by the United States to Israel should be used only for defensive purposes (AIPO, July 1982).

Military aid to Israel has been increasingly mentioned in the polls as a way for the U.S. government to adopt sanctions: Early in August 1982, when Gallup asked the question, "In the past few days Israel has sent its military forces into West Beirut to try to expel the PLO military forces. What should the U.S. government do?" 16 percent opted for criticizing Israel and pressuring the Israelis diplomatically, and 43 percent for the possibility of using military aid as a sanction (Newsweek/Gallup, August 4–5, 1982).

We are indebted to the following organizations for permission kindly granted to publish their research results:

AIPO—American Institute of Public Opinion (Gallup), USA
CBS—CBS News Poll, USA
Demoskopie—Institut für Demoskopie Allensbach, West Germany
Elsevier—Elsevier's Magazine, Netherlands
EMNID—EMNID Institut GmbH, West Germany
GMA—Gallup Markedsanalyse A/S, Denmark
Harris—Louis Harris and Associates Ltd., USA
MORI—Market and Opinion Research International, Great Britain
Newsweek—Newsweek Poll/Gallup, USA
NIPO—Netherlands Instituut voor de Publieke Opinie, Netherlands
SOC—Social Surveys Ltd. (Gallup), Great Britain

In some polls, the questions were submitted only to those respondents in the sample who were aware of the situation in the Middle East. In such cases we have included either the introductory question used in the survey or a note reading, "Based on 'aware' group."

The results are based on national representative samples of adults. The dates given with the tables are, as far we know, the dates on which the interviews were conducted. Where the tables do not add up to 100 percent, the apparent discrepancy is due to the percentages having been rounded. When the size of the random sample was given in the original publication, this information is given with the tables. Breakdowns and additional information may be obtained by applying to: The Polls Archives, Baschwitz Institute, University of Amsterdam, 100-102 Weteringschans, 1017 XS Amsterdam, Netherlands.

Sympathies

USA—AIPO: Have you heard or read about the situation in the Middle East? [If yes] In the Middle East situation are your sympathies more with Israel or more with the Arab nations?

	Israel	*Arab Nations*	*Neither*	*No Opinion*	
June 1967	56%	4%	25%	15%	
Jan. 1969	50	5	28	17	
Mar. 1970	44	3	32	21	
Oct. 6–8, 1973	47	6	22	25	
Oct. 19–22, 1973	48	6	21	25	
Dec. 7–10, 1973	54	8	24	14	
Jan. 1975	44	8	24	24	
Apr. 1975	37	8	24	31	
June 3–6, 1977	44	8	28	20	*n* = 1311
Oct. 14–17, 1977	46	11	21	22	*n* = 1520
Feb. 1978	33	14	28	25	
Apr./May 1978	44	10	33	13	
Sep. 22–25, 1978	42	12	29	17	
Nov. 10–13, 1978	39	13	30	18	*n* = 1210
Jan. 5–8, 1979	42	15	29	14	*n* = 1405
July 1981*	49	10	20	21	*n* = 551
Jan. 8–11, 1982	49	14	23	14	
June 11–14, 1982	52	10	29	9	*n* = 1399
July 11–14, 1982	41	12	31	16	*n* = 1389
Sep. 22–23, 1982*	32	28	21	19	*n* = 605

* Newsweek/Gallup Poll; source: Newsweek, October 4, 1982.

USA—Newsweek Poll/Gallup: Compared to a year ago, would you say you are more sympathetic or less sympathetic to the Israeli/Palestinian position?

	Israeli Position				*Palestinian Position*				
	More	*Less*	*Same*	*DK*	*More*	*Less*	*Same*	*DK*	
Feb. 1978	27%	34%	19%	20%					
July 1981	29	37	18	16	22%	36%	21%	21%	
Aug. 4–5, 1982	32	41	15	12	28	40	18	14	*n* = 752
Sep. 22–23, 1982	24	51	10	15	39	27	15	19	*n* = 605

Source: Newsweek, Aug. 16, 1982, Oct. 4, 1982.

USA—CBS News Poll: Some people think the United States should pay more attention to the demands of the Arabs, even if it means antagonizing Israel, while other people think the U.S. should give its strongest support to Israel, even if it means antagonizing the Arabs. If these were the only two choices, what should the U.S. do—pay more attention to the Arabs, or give our strongest support to Israel?

	Israel	*Arabs*	*Both/Neither*	*No Opinion*	
June 1980	43%	23%	9%	25%	
June 26–27, 1982	41	21	12	26	*n* = 985

Great Britain—SOC: Whom are your sympathies with in the present Middle East dispute: Israel or Egypt and other Arab countries?

Interview Date	*Israel*	*Egypt/Arabs*	*Neither*	*DK*
June 5–11, 1967	55%	2%	27%	16%
June 12–18, 1967	59	4	22	15
Jan. 2–7, 1969	41	8	29	21
Feb. 28–Mar. 4, 1969	53	6	21	20
Aug. 7–12, 1969	36	4	31	29
Sep. 11–14, 1969	41	5	35	20
Nov. 13–16, 1969	34	5	32	29
May 1970	43	5	29	23
Oct. 1–4, 1970	33	8	37	22
Oct. 19–24, 1972	40	5	30	25
Apr. 27–May 1, 1973	34	7	35	24
Dec. 12–16, 1973	35	10	36	19
May 23–29, 1974	39	6	34	21
July 9–14, 1975	33	8	31	28

Great Britain—MORI/Survey for The Main Event Ltd. on behalf of the PLO: Has Israel's invasion of the Lebanon made you more sympathetic to Israel, less sympathetic, or do you feel about the same? And has Israel's invasion of the Lebanon made you more sympathetic to the Palestinians, less sympathetic, or do you feel about the same?

	More	*Less*	*About the Same*	*DK*	
Aug. 14–17, 1982					
Sympathetic to Israel	10%	34%	48%	8%	$n = 1061$
Sympathetic to Palestinians	23	15	52	10	

Great Britain—NOP: I'd like to ask you about the Arab-Israeli war. Which side are you most inclined to support—the Arabs or the Israelis?

	Arabs	*Israelis*	*Neither*	*Both*	*DK, No Opinion*	
Mid Apr. 1973	2%	23%	34%	2%	39%	$n = 1798$
May 1973	5	30	35	3	27	$n = 514$
Oct. 13, 1973	5	47	33	N.A.	15	$n = 1092$
Nov. 25, 1973	7	43	31	N.A.	19	$n = 1885$

Great Britain—NOP/Survey for Eurabia Ltd.: Which side do you support in the Arab-Israeli conflict?

	Arabs	*Israelis*	*Neither*	*Both*	*DK, No Opinion*	
Apr. 22–28, 1976	7%	36%	31%	8%	18%	$n = 2039$

Netherlands—NIPO: Do you consider yourself neutral in the conflict between Israel and the Arab states, or do you take sides? Are you on the side of Israel or on the side of the Arab states?

	Israel	*Arabs*	*Neutral*	*No Opinion*	
June 1967	67%	0%	26%	7%	
July 1967	64	0	25	11	$n = 483$
Apr. 1968	52	0	31	17	$n = 460$
Dec. 1969	51	1	27	21	
Jan. 1973	36	1	59	4	
Dec. 1973	40	1	51	8	
Nov. 1974	37	3	56	4	$n = 834$

Netherlands—NIPO/Survey for Elsevier's Magazine: How sympathetic are you to the Palestinians? How sympathetic are you to the Israelis?

	Highly	*Somewhat*	*Not at All*	*No Answer*	
Sympathetic to Palestians					
Aug. 1979	32%	55%	8%	5%	
June 1982	28	53	15	4	
Sep. 22, 1982	29	53	15	3	$n = 417$
Sympathetic to Isrealis					
Aug. 1979	57%	36%	3%	3%	
June 1982	41	45	12	3	
Sep. 1982	35	49	16	1	$n = 417$

Source: Elsevier's Magazine, July 10, 1982; Oct. 2, 1982.

Denmark—GMA: Who in your opinion is right in the struggle between Israel and the Arab countries?

	June 1967	*Jan. 1969*	*May 1970*	*Jan. 1974*	*Mar. 1978*	*May 1981*	*Aug. 1982*
Israel	56%	42%	43%	37%	28%	25%	28%
Arab countries	2	5	6	7	8	10	14
Don't know	42	53	51	56	64	65	58

West Germany—Demoskopie: A question about the conflict between Israel and the Arab nations: On whose side are you—more on the side of the Israelis or more on the side of the Arabs? (1970, 1971, 1973, 1978, and 1981) Whom are your sympathies with in the Middle East conflict: with the Israelis, with the Arabs, or with neither of the two parties? (Apr. 1973)

A question on the war between Israel and the Arabs: Whom are your sympathies with in this war? (Oct. 1973)

	Israelis	*Arabs*	*Neither*	*No Opinion*	
May 1970	45%	7%	32%	16%	
Apr. 1971	43	8	29	20	
Apr. 1973	37	5	37	21	$n = 2000$
Oct. 17–18, 1973	57	8	25	10	$n = 500$
Apr. 1978	44	7	33	16	$n = 2000$
May 1981	24	16	41	19	$n = 2585$

Palestinians and the PLO

USA—Harris: On the issue of a homeland for the Palestinians, would you favor having Jordan and parts of the West Bank under Jordan made into a Palestinian homeland, or making the West Bank an independent Palestinian state, or having the Palestinians live where they do now in Jordan, on the West Bank, in the Gaza strip, and in Lebanon, with full rights in each of those places?

Jan. 18–19, 1978; $n = 1191$	
In Jordan and West Bank	8%
Independent state on West Bank	20
Where they live now	38
Not sure	34

USA—AIPO: As you may know, one of the major questions in the Middle East situation concerns the Palestinian people. Do you think a separate, independent Palestinian nation should be established or do you think the Palestinians should continue to live as they do now in Israel and the neighboring Arab nations?

Based on "Aware" Group	*Establish Separate Nation*	*Continue as They Do Now*	*Other Responses*	*No Opinion*	
Oct. 14–17, 1977	36%	29%	3%	32%	$n = 1520$
July 23–26, 1982	46	23	31		$n = 1389$

USA—Newsweek Poll/Gallup: The Palestinians have fought for many years with Israel for the establishment of a Palestinian state. Israel says that such a Palestinian state would be a threat to its security. Should the United States favor a Palestinian state or not? Should the United States talk directly with the PLO as the representative of the Palestinian people or not?

Favor Palestinian State	*Yes*	*No*	*DK*	
Apr. 1980	33%	37%	30%	
July 1981	31	44	25	
Aug. 4–5, 1982	37	45	18	$n = 752$
Talk with PLO				
Aug. 4–5, 1982	48%	42%	10%	$n = 752$

Source: Newsweek, Aug. 16, 1982.

USA—Newsweek Poll/Gallup: The Camp David accords call for negotiations on Palestinian autonomy over the Israeli-occupied West Bank. Which of these proposals for the West Bank would you prefer to see implemented?

	Sep. 1981	*Sep. 22–23, 1982*	
	Jews	*Jews*	*National*
Israeli sovereignty with military and civil control by Israel	29%	19%	9%
Israeli sovereignty with military control by Israel but civil control by the Palestinians themselves	32	39	12
Returning West Bank to Jordan sovereignty and making it a demilitarized zone	14	16	31
An independent Palestinian state	9	7	23
Don't know	16	19	25

Source: Newsweek, Oct. 4, 1982.

Netherlands—Elsevier/NIPO: Are the Palestinians entitled to a state of their own?

	Entitled	*Not Entitled*	*No Answer*
Aug. 1979	91%	4%	5%
June 1982	88	7	5

Source: Elsevier's Magazine, July 10, 1982.

Netherlands—Elsevier/NIPO: Does the PLO pose a greater menace to world peace than Israel does?

	Israel Greater Menace	*PLO Greater Menace*	*Both Parties Equally*	*Neither Party in Any Way*	*No Opinion*
Aug. 1979	9%	30%	36%	18%	7%
June 1982	16	27	35	17	5

Source: Elsevier's Magazine, July 10, 1982.

Great Britain—NOP/Survey for Eurabia Ltd.: Do you think the PLO should be involved in future Middle East peace negotiations?

Based on "Aware" Group	*Yes*	*No*	*DK*	
Apr. 22–28, 1976	49%	33%	19%	$n = 2039$

Great Britain—MORI/Survey for The Main Event Ltd. on behalf of the PLO: Do you think the PLO should be included in any future Middle East peace talks or not? Do you think Britain should recognize the PLO as the representative of the Palestinian people or not?

Aug. 14–17, 1982; *n* = 1061	*Yes*	*No*	*DK*
Include PLO	61%	24%	15%
Recognize PLO	39	36	25

Great Britain—SOC: Do you think the Israelis should withdraw to their original frontiers, return most of the Arab territory it now occupies but retain some of the territory like Jerusalem and the Gaza Strip, keep hold of all or most of the territory they have taken over?

	Withdraw	*Return Terr.*	*Keep Terr.*	*DK*
Oct. 1967	13%	21%	44%	22%
July 25–28, 1968	12	22	41	25
Aug. 7–12, 1969	13	17	28	41
Sep. 1970	18	26	26	30
Oct. 1–4, 1970	20	19	24	37
Nov. 5–8, 1970	17	22	23	38
Oct. 19–24, 1972	14	20	27	40
Dec. 12–16, 1973	26	27	18	29

Great Britain—SOC: I am going to read out some statements that have been made about the Middle East and I would like you to tell me whether you agree or disagree with each of them.

	Agree Strongly	*Agree Slightly*	*Neither Agree/ Disagree*	*Disagree Slightly*	*Disagree Strongly*
Israel should remove all its settlements from the Arab territories they occupied after the 1967 war.					
Mar. 8–13, 1978	12%	19%	42%	16%	11%
Sep. 27–Oct. 2, 1978	20	27	32	12	8
The Arabs should agree to recognize Israel.					
Mar. 8–13, 1978	37	33	28	1	1
Sep. 27–Oct. 2, 1978	45	33	20	1	1
Israel should agree to withdraw from the Arab territories they occupied after the 1967 war.					
Mar. 8–13, 1978	14	23	41	14	7
Sep. 27–Oct. 2, 1978	23	27	33	11	6
The Palestinians should be free to establish a state of their own in the parts of Palestine occupied by Israel after the 1967 war.					
Mar. 8–13, 1978	16	26	44	9	6
Sep. 27–Oct. 2, 1978	18	32	39	6	5

Great Britain—NOP/Survey for Eurabia Ltd.: When the state of Israel was set up about 30 years ago in Palestine, large numbers of Palestinians who lived there became refugees and are still living in camps in other Arab countries. I want to ask you about some suggestions for dealing with this problem, and whether or not they would be acceptable in your opinion.

Apr. 22–28, 1976; $n = 2039$	*Acceptable*	*Unacceptable*	*DK*
Firstly, leave the Palestinian refugees where they are	25%	55%	21%
Allow the refugees to return and divide Israel into separate Arab and Jewish states.	43	38	19
Allow the Palestinian refugees to return home to become part of a new state in Palestine but giving equal rights to Jews and Arabs.	75	11	14
Allow the refugees to return and take over the whole State of Israel from the Isrealis.	3	82	15

Great Britain—MORI/Survey for The Main Event Ltd. on behalf of the PLO: When the state of Israel was created in 1948, large numbers of Palestinians who lived there became refugees and are still living in camps in other Arab countries. I want to ask you about some suggestions for dealing with this problem and whether or not they would be acceptable in your opinion? Now, would it be acceptable or not to . . .

Aug. 14–17, 1982; $n = 1061$	*Acceptable*	*Not Acceptable*	*DK*
Leave the Palestinian refugees where they are.	26%	54%	20%
Allow the Palestinian refugees to return and for Israel, the West Bank and Gaza to be divided into two separate Jewish and Arab states.	55	21	23
Allow the Palestinian refugees to return, and create a new state, incorporating Israel, the West Bank and Gaza, of which Jews and Arabs would be citizens with equal political and religious rights.	60	22	18

Arms Supplies

USA—Harris: Do you think the U.S. was right in sending planes and other military supplies to Israel or do you think we should have taken a different course?

	Right to Send	*Take Different Course*	*Not Sure*
1967	35%	39%	26%
Oct. 26–29, 1973	46	34	20

USA—Harris: As you know, the U.S. has sent planes, tanks, artillery and other weapons to arm Israel. The Russians have sent similar military supplies to Egypt and Syria. In general, with the Russians arming Egypt and Syria, do you think the U.S. is right or wrong to send Israel the military supplies it needs?

	Right to Send	*Wrong to Send*	*Not Sure*	
Jan. 1975	65%	21%	14%	
May 1975	62	18	20	$n = 1428$

USA—AIPO: Do you think the U.S. should supply arms and material to Israel/the Arabs?

	Israel			*Arms to Arabs*		
Based on "Aware" Group	*Yes*	*No*	*No Opinion*	*Yes*	*No*	*No Opinion*
Oct. 19–22, 1973	37%	49%	14%			
Mar.5–6, 1978	28	54	18	15%	68%	17%

USA—Harris: President Carter wants to go ahead with the sale of military aircraft to Israel, Saudi Arabia and Egypt for $4.8 billion. He wants to sell Israel 15 F-15s and 75 F-16s, the most modern jet planes the United States produces. Egypt would receive 50 F-5Es, which are effective short-range fighter/bombers. Saudi Arabia would receive 60 F-15s, a sophisticated long-range fighter. The President wants to sell all of the planes as one package: either all three countries receive their planes or none receives them? Do you favor or oppose this U.S. sale of military planes to Israel, Egypt and Saudi Arabia?

Apr. 19–20, 1978; *n* = 1199	
Favor	26%
Oppose	66
Not sure	8

Now let me ask you about the sale of military planes to each individual country. Do you favor or oppose the sale of these military planes to (read list)?

Apr. 19–20, 1978; *n* = 1199	*Favor*	*Oppose*	*Not Sure*
Israel	28%	64%	8%
Egypt	20	71	9
Saudi Arabia	18	73	9

USA—Harris: Let me read you some statements about the U.S. Senate upholding President Reagan on the sale of the sophisticated radar aircraft, AWACS, to Saudi Arabia. For each, Tell me if you agree or disagree.

Oct. 28–Nov. 3, 1981; *n* = 1249	*Agree*	*Disagree*	*Not Sure*	
The US should not give, lend, or sell sophisticated military equipment to countries such as Saudi Arabia without having control over how such equipment will be used.	81%	16%	3%	
All in all, do you favor or oppose the sale of AWACS to Saudi Arabia?	*Favor*	*Oppose*	*Not Sure*	
Oct. 28–Nov. 3, 1981	42%	48%	10%	*n* = 1249

USA—AIPO: Some people say the U.S. should require that all weapons sent by the U.S. to Israel should be used only for defensive purposes. Other people say that Israel should be able to use these weapons in any way they feel is necessary. Which point of view comes closer to your own?

Based on "Aware" Group	*Defensive Use Only*	*Any Way Necessary*	*No Opinion*	
July 23–26, 1982	64%	26%	10%	*n* = 1389

USA—Newsweek Poll/Gallup: Do you think U.S. aid to Israel should be suspended or reduced in order to force a pullout of Israeli forces from Lebanon?

	Yes	*No*	*DK*	
Sep. 22–23, 1982	50%	38%	12%	*n* = 605

Source: *Newsweek*, Oct. 4, 1982.

Great Britain—SOC: America may give armed support to Israel, apart from anything decided by the United Nations. Should Britain follow America's lead or should we refuse to get our armed forces involved?

	Follow America	*Refuse to Get Involved*	*DK*
May 29–June 4, 1967	17%	71%	12%

Great Britain—NOP/Survey for Eurabia Ltd.: Do you think Britain should supply arms to:

	Arabs Only	*Israelis Only*	*Both*	*Neither*	*DK*
Apr. 22–28, 1976; *n* = 2039	1%	5%	21%	68%	5%

Great Britain—MORI/Survey for The Main Event Ltd. on behalf of the PLO: Do you agree or disagree with Britain's decision to impose a total embargo on arms sales to Israel?

	Agree	*Disagree*	*DK*	
Aug. 14–17, 1982	63%	26%	11%	*n* = 1061

Invasion of Lebanon

USA—AIPO: Israel recently began military operations in Southern Lebanon to stop Palestinian artillery attacks on settlements in Israel. Do you approve or disapprove of this action by Israel?

Based on "Aware" Group	*Approve*	*Disapprove*	*No Opinion*	
June 11–14, 1982	40%	35%	25%	*n* = 1399

USA—Harris: Israel and the U.S. have agreed that the solution in Lebanon should be to have all non-Lebanese military power—Israel, Syria and PLO—leave that country and allow the Lebanese to rule themselves and to control their own country militarily. Do you favor or oppose this objective?

June 18–22, 1982	
Favor	76%
Oppose	14
Not sure	10
	n = 1255

USA—Harris: Another part of the solution would be to have a special military force occupy the 25-mile buffer zone in southern Lebanon bordering Israel. This force—like the one on the Sinai on the Egypt-Israel border—would come from several countries, including the U.S. Would you favor or oppose the U.S. sending a small number of troops to Lebanon as part of a peace-keeping force there?

June 18–22, 1982	*Total*	*Men*	*Women*
Favor	41%	49%	33%
Oppose	54%	48%	61%
Not sure	5%	3%	6%

USA—CBS News Poll: Some people say Israel is right to fight in Lebanon in order to stop the PLO—the Palestine Liberation Organization. Others say that Israel is wrong to go into Lebanese territory. Do you think that Israel is right or wrong to fight in Lebanon?

Based on "Aware" Group	*Right*	*Wrong*	*Other (vol)*	*No Opinion*	
June 26–27, 1982	34%	38%	4%	24%	*n* = 985

USA—CBS News Poll: What should the United States do? Should the U.S. government publicly support Israel, should it say or do nothing, should it publicly criticize Israel, or should it reduce its military aid to Israel?

Based on "Aware" Group	*Support*	*Say Nothing*	*Criticize*	*Reduce Aid*	*No Opinion*	
June 26–27, 1982	20%	32%	7%	24%	17%	*n* = 985

USA—Newsweek Poll/Gallup: In the past few days Israel has sent its military forces into West Beirut to try to expel the PLO military forces. What should the U.S. government do?

	Aug. 4–5, 1982
Support Israel's actions	16%
Criticize Israel and pressure Israelis diplomatically	16
Suspend military aid to Israel for the time being	27
Permanently cut off military aid to Israel	16
Do nothing	15
Don't know	10
	n = 752

Source: Newsweek, August 16, 1982.

USA—Newsweek Poll/Gallup: Which of the following comes closest to your view? (Asked only of those who had heard about the massacre in Beirut)

Sep. 22–23, 1982; *n* = 605	
Israel cannot be held responsible for the massacre because it was carried out by Lebanese Christian militia.	8%
Israel must bear partial responsibility because its troops had taken control of the area where the massacre occurred.	49%
Israel is very much responsible because it let Christian militia soldiers into Palestinian camps.	32%
Don't know	11%

Source: Newsweek, October 4, 1982.

USA—Harris: As you know, Israel moved its troops into Lebanon to try to eliminate the PLO and Syrian military bases there which had been used to shell northern Israel. If the conflict ends with all foreign military powers—Israeli, Syrian, and PLO—finally getting out of Lebanon, do you think the Israeli move into Lebanon was justified or not?

	Justified	*Not Justified*	*Not Sure*	
July 1982	44%	36%	20%	
Aug. 5–10, 1982	43	42	15	*n* = 1254

Netherlands—Elsevier/NIPO: Do you have understanding for the invasion by the Israelis?

	Much Understanding	*Little Understanding*	*No Understanding at All*	*No Answer*
June 1982	24%	40%	33%	2%

What do you feel Israel should have done?

	Refrain from Invading	*Occupy Strip of Land*	*Advance on Beirut*	*No Answer*
June 1982	47%	30%	16%	7%

Source: Elsevier's Magazine, July 19, 1982.

Great Britain—MORI/Survey for The Main Event Ltd. on behalf of the PLO: Do you support or oppose Israel's invasion of the Lebanon?

	Support	*Oppose*	*DK*	
Aug. 14–17, 1982	22%	57%	21%	*n* = 1061

On balance, do you support or oppose Israel's tactics in cutting water and food supplies to West Beirut?

	Support Israel's Tactics	*Reject Israel's Tactics*	*DK*	
Aug. 14–17, 1982	9%	83%	8%	*n* = 1061

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The Implicit Assumptions of Television Research: An Analysis of the 1982 NIMH Report on *Television and Behavior*

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In 1969, Senator John Pastore called on the surgeon-general to organize a research effort designed to discover whether television violence increases aggressiveness in children. The surgeon-general appointed a Scientific Advisory Committee that commissioned research on the topic and in 1972 issued a report concluding that television violence does indeed cause aggression, at least in some children. In 1979, some researchers suggested to the surgeon-general that it would be useful to

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Abstract The authors analyze some of the assumptions underlying most current research on television. They emphasize the dependence on (1) an individual rather than an institutional level of analysis; (2) a model of research utilization that pays little explicit attention to where sources of leverage lie for changes in programming; (3) extremely simple models of the selection processes associated with different levels of television viewing; and (4) uncritical appraisals of the consequences of effects that many would call small or modest. These issues are illustrated by a general discussion of the NIMH report on *Television and Behavior* and specific discussion of "mainstreaming" and the effects of television violence.

In 1972, POQ's editors invited Leo Bogart to prepare an extended review article of the Surgeon-General's Study of Television and Social Behavior (POQ 36:491–521). When the 10-year follow-up study was released by NIMH in 1982, the editors asked Thomas D. Cook, a distinguished psychologist noted for his research on television, to perform the same function.

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assess the newer literature. According to the foreword to *Television and Behavior: Ten Years of Scientific Progress and Implications for the Eighties* (NIMH, 1982) he agreed that this would "provide opportunity to be more definitive regarding television's causal influences on violent and aggressive behaviors of viewers as well as to address an increasing number of questions about the medium's impact on viewers' functioning." And so a new research effort began under the direction of David Pearl at the National Institute of Mental Health (NIMH).

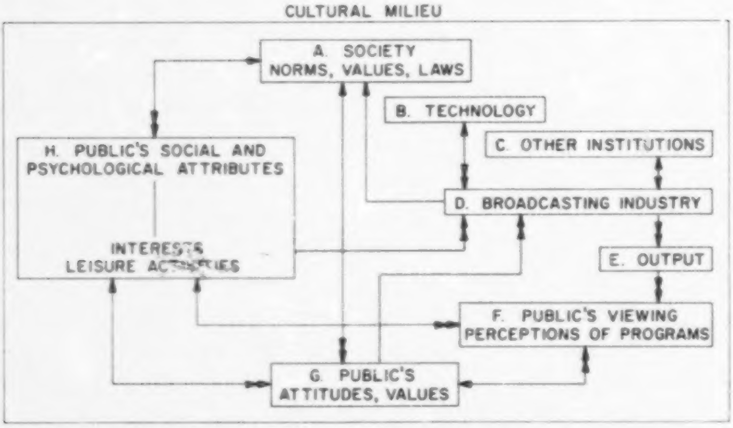
Its aims were to review the most recent literature on television entertainment—"the kinds of programs watched by most of the audience, most of the time," and "to elucidate research findings and their implications for public health and future research." The foreword also notes that the report, while "relevant to public policy . . . makes no recommendations and does not issue specific prescriptions." Yet the foreword also adds, "We would anticipate . . . that persons bearing responsibility for policy and for television industry practices would be interested in the findings for use in decision making. Also the report should be of substantial help to parents and others who seek to know of both the positive and adverse effects of the medium and of the ways in which they can influence them." Thus, readers are encouraged to believe that the report has action implications, but it is left up to them to decide what they are.

The report was released in 1982 in two volumes about five months apart. The first volume is a summary, and the second contains the 24 literature reviews on which the first volume is based. Two related themes continually occur throughout the NIMH report: that current entertainment programming has largely negative effects, and that researchers have identified alternative types of programming that have more prosocial effects. We want to examine the principal negative claims in the report: that television violence causes aggression, and that a distorted world presented on television causes heavy viewers to see the real world as more hostile and scary than it really is. We also want to explore what is known about prosocial effects that might be brought about, especially concerning children's programming and the use of television for promoting public health.

For each of these issues, we try to assess the degree of scientific support for the report's major findings, and we also examine the conditions under which the findings might be used to modify programming. This last emphasis clashes with the report's stated aim of presenting the "facts" and letting readers draw their own implications. However, the report does make the claim that current programming has mostly negative effects that could be reversed with different programming. This claim has fairly obvious action implications, the report's disclaimer notwithstanding. We probe these implications, and deal especially with how the report's major claim is related to the sources of leverage that currently exist for making programming changes. But before exploring these issues, we want to analyze the scope of the NIMH report in order to force out some of its implicit assumptions.

The Scope of the Report

The report's scope can best be understood by comparing its coverage of televison research with the coverage implied by Himmelweit's (1980) comprehensive model of the field, which is presented in Figure 1. Himmelweit postulates a set of elements that determine both how television affects viewers and society and how viewers and society affect television. These are: the norms of a country; the institutions that impinge on its broadcast industry (e.g., in the U.S. these would include the FCC, Congress, and various advocacy groups); the structure of the broadcast industry itself (e.g., networks, local stations, production companies); the technological state of the art (e.g., the availability of multichannel cable); the industry's outputs (largely the advertisements and news and entertainment shows that are produced); viewer perceptions of these outputs; the attitudes and values of the viewing audience; and the stable social and psychological attributes of specific groups within the audience (e.g., race, age, level of education, etc.).


CULTURAL MILIEU
A. SOCIETY NORMS, VALUES, LAWS
B. TECHNOLOGY
C. OTHER INSTITUTIONS
H. PUBLIC'S SOCIAL AND PSYCHOLOGICAL ATTRIBUTES
D. BROADCASTING INDUSTRY
INTERESTS LEISURE ACTIVITIES
E. OUTPUT
F. PUBLIC'S VIEWING PERCEPTIONS OF PROGRAMS
G. PUBLIC'S ATTITUDES, VALUES

↕ Direction of strong influences. ↑ Direction of some influences.

SOURCE: Himmelweit (1980).

Figure 1. Himmelweit's Model of Television and Its Influences

With the help of Himmelweit's model we can trace the priorities in previous television research. Most of it explored how the industry's output influenced viewer perceptions and how these perceptions affected attitudes and behavior. The causal agent was usually television considered globally, and any differentiation of content was largely to explore dramatic violence as a presumed cause. Children were studied most often, usually as individuals. Institutions rarely came under study. Thus, only a part of Himmelweit's model was explored; and in her judgment, it is a part that has less influence on the socialization of children than general cultural norms or the social beliefs of the particular groups to which children belong.

The NIMH report re-creates one of the past priorities, since nearly all of Volume 2 concerns children—how television relates to their information processing, imagination, affective development, academic achievement, and critical viewing skills; how it relates to aggression, sex-role development, the acquisition of consumer habits, and learning about sex. But another past priority is not re-created, since only about 10 percent of Volume 1 and 20 percent of Volume 2 deal explicitly with violence, though the topic arises as a minor theme in many other places. Instead, the report places relatively more stress on prosocial programming, thereby contributing to an enlargement of the scope of television research along with Comstock et al. (1978) and Withey and Abeles (1980).

But contrast this enlarged scope with the elements from Himmelweit's model that are left largely or totally unexplored. Where in the report are the extended discussions of the structure and functions of the broadcast industry so that one can understand better the types of programs that are broadcast? The report does contain two (of 24) chapters on the industry and its place in society. But this strikes us as an underemphasis when exploring programming content that is presumably influenced by a complex set of economic, political, sociological, managerial, and historical forces. To be sure, the industry is difficult to study. For example, the networks resent outsiders from academe, who are thought to have too little appreciation of the pressures on broadcasters and who often seem to hold alien conceptions of television's function. Nonetheless, research on the industry is vital for anyone who wants to understand how programming decisions are made or who wants to identify research questions with leverage either for theoretical understanding or for programming changes.

A second part of Himmelweit's model not explicitly covered in the NIMH report concerns the institutions that interface with television. Television is financed by companies that want to advertise, and a powerful industry has developed to commission, produce, and market these advertisements. We know relatively little about how advertisers and the advertising industry influence television and are influenced by it. Another institution affecting television content is the Federal Communications Commission (FCC), which determines the issuance and renewal of station licenses. We know relatively little about how the FCC actually operates, what the forces are sustaining it, and under what conditions it will move against stations because of their programming. Other institutions influencing the content of television include Congress, the courts, advocacy groups, and public opinion in general. Such institutions are rarely mentioned in the NIMH report.

Even if we consider only television programming, the NIMH report still has a restricted scope. In particular, little consideration is given to public affairs programming or to the medium's role in stimulating consumption, each of which may have important social-structural implications. What if, for instance, news coverage on television (a) makes a media-genic style necessary for political candidates to be selected and elected; (b) weakens political parties and local machines because skillful candidates can go more directly to voters; (c) plays a major role in the setting of political agendas; (d) influences voter turnout; or (e) modifies public opinion through the repeated presentation of dramatic footage about political events (e.g., as in the Vietnam War)? The NIMH report is not as comprehensive as its title, *Television and Behavior*, suggests. It would be more accurately described as a report on "The Psychological Consequences of Television Entertainment at the Individual Level, with Special Reference to Children."

Why does the report have these emphases? We can only speculate. They probably reflect the momentum of received research traditions, which had to play a major role in the report once the decision was made to review past research rather than fund new studies. Also, the emphases may reflect the NIMH sponsorship with the institute's inevitable preference for psychological variables. The composition of the Advisory Committee, whose members helped choose the topics and reviewers, may also have played a role. Of its seven members, we would classify five as having an exclusive orientation toward the psychological, and the other two as exhibiting the same paradigmatic bias in much (but not all) of their work. The committee had no economists or sociologists of the media, no one trained in political science, administrative studies, or policy sciences, and no one from the networks or other parts of the broadcast industry.

But to be fair, we should note the difficulty of studying television in the way that Himmelweit's model suggests. Such research would have to be associated with reciprocal causal influences, multiple institutions, and different levels of analysis. Anyone who appreciates the difficulties of studying even one-way causal influences from television to children will blanch at exploring all of Himmelweit's model, however comprehensive it may be in capturing the reality of television for those who want to explain it or for those who want to change it. We should also note that, within the report, both Chaffee and McLeod et al. lament the dearth of studies at the family level. This means that some authors did recognize the need to move beyond individuals to higher-order aggregates. However, these are only two sets of authors and they limit their analysis to the family, leaving out of consideration political, economic, and other social institutions. Thus, they do not seriously challenge our view that the NIMH report is less broad in scope than its title, *Television and Behavior*, suggests.

Prosocial Programming for Children

Throughout the NIMH report runs the message that television could portray themes other than adventure, violence, and titillation; that it could instead portray cooperation, altrusim, and self-control; that it could promote healthier lifestyles; and that it could even model proven strategies for coping with life's common problems. A fundamental change in television's functions is implied in this advocacy. We move from the primacy of delivering potential customers to advertisers at the lowest cost per thousand to the primacy of using television to entertain while also promoting physical and emotional health and embodying the more interpersonally binding American values. Make no mistake: A fundamental challenge implicit in the NIMH report is to the exclusively commercial rationale for entertainment television in the U.S. today. To examine the prosocial focus in the report, we now turn to the first of our two substantive domains: prosocial programming for children.

The major chapter on this topic is by Rushton, who succinctly summarizes research from experiments in both laboratory and field settings that supports the proposition that television can teach children and adolescents behaviors widely recognized as altruistic, friendly, indicative of self-control, and effective in coping with fear. Prosocial issues are also discussed in other chapters: Dorr suggests that television can teach empathy and the ability to recognize emotions; Singer claims that television content can stimulate the imagination, but that cartoons do not; and Rubinstein and Sprafkin report an original study which they interpret as showing that a prosocial television diet can induce prosocial behavior in persons aged 8 to 18 who are institutionalized because of poor social functioning.

Dorr and Singer admit that there is not much evidence for their hypotheses, while the research of Rubinstein and Sprafkin is preliminary and difficult to interpret. However, Rushton's case is based on many studies that converge impressively in their results. The television stimulus in these studies consists of (a) films that were created especially for research; (b) specific shows from the "Mister Rogers" or "Sesame Street" series; or (c) special viewing diets created by restricting the child's exposure to prosocial programs taken from tape libraries. The reader might well wonder what effects such shows would have in home settings where it may be easier for attention to wander, where antisocial models and events are interspersed with the prosocial, and where competing activities are available in the form of other channels, video recordings, or play. From the network perspective, the most important attribute of programs is their ability to attract and hold an audience, despite competition for the child's attention.

While holding the audience is the end of commercial broadcasting, for most researchers it is merely the means. This disjunction of perspective is very salient in Rushton's chapter, for it contains no mention of how attractive a prosocial diet would be to children of different ages. The only authors to consider the issue are Rubinstein and Sprafkin, who conclude from their own study that attention did not differ between adolescents exposed to a typical television diet and adolescents exposed to a prosocial one. While an important first step, this still does not speak to the prior question of whether children will spontaneously *select* prosocial programs, especially when there are alternatives that include action-packed shows and a variety of nontelevision activities. Individual networks probably fear that, if they introduced more prosocial programming, they would be at a competitive disadvantage. Their need for large audiences dictates research not just on the effects of prosocial content, but also on the ability of such programming to first capture, and then hold large audiences. Without such research, the networks have every reason to fear prosocial television in the sense we are using the term.

This can be seen by examining "Mister Rogers," "Sesame Street," and the "Electric Company." They are widely acclaimed as prosocial programs; yet it is noteworthy that none of the networks has tried to create such programming. On network television, children's programs continue to be almost nonexistent on weekdays and to feature cartoons on Saturdays. Indeed, one of the few live shows—"Captain Kangaroo"—was recently pushed back to a timeslot when we would all like our children to be asleep and, not surprisingly, the ratings soon killed the show. Nowhere in the NIMH report is there an extensive analysis of the economics of the media and of the paucity of incentives for the networks to improve children's programming.

We probably understand at a very coarse level the reasons for the networks' lack of interest. Children are captivated by cartoons, which cost relatively little to produce; in determining purchasing decisions children play a role, but it is not usually the major one; the child market is shrinking because of demographic trends; and children's abilities and interests change markedly from one narrow age band to another, limiting the potential audience for most shows. In this last respect, action-packed cartoons are somewhat of an exception, since they hold children of 4, 8, and 12 in a way that no program can that is aimed at the specific needs of a particular age group. For all these reasons, we have to conlude that it is no accident that the productions of the Children's Television Workshop have not been extensively imitated by the networks.

In theory, the difficulty of having prosocial material broadcast can be circumvented through regulation, subsidies to the networks, or the eventual control by the federal government of a children's channel in the narrowcast era. Each of these has distinct drawbacks, is highly controversial, and, in the short run at least, is only a remote practical possibility. The omnipresent reality is: Even if prosocial programming were effective with children and adolescents when it was watched, why should the networks broadcast it? Certainly, they do not seem worried by the head-to-head competition with prosocial programs on public television on Saturday mornings. We presume that the networks have examined the ratings and are not moved to modify their current practices. They have probably also examined the ratings of the current children's channel on cable television and are aware of the relatively small audiences, the financial losses being incurred, the steady drift in the programming toward action-packed cartoons, and the escalating talk about the possibility of advertising in the near future. With these advertisers will come pressure to increase audience size so that the channel may come to look more and more like network television. To achieve large audiences will require a departure from the fundamental tenet of prosocial television for children: That each age group has specific needs and skills that require different types of programming, much as different types of textbooks are needed at each grade in schools. The problem with such wisdom from developmental psychology is that to implement it would realize small audiences and so be poor business.

A final note is worth making about what is absent from the report's discussion of prosocial programming. The cited evidence comes almost entirely from short-term experiments which show what exposure to prosocial content *might* achieve if it were watched. No longitudinal studies are reported which probe the effects of differences in spontaneous exposure to prosocial content, both during children's viewing hours and at other times, when indeed most viewing by children actually takes place. The absence of such longitudinal studies may be due to researchers' doubts about whether there has been enough prosocial content to make an analysis meaningful, or it may reflect the researchers' personal conviction that the results are self-evident. However, all the inferential traps that beset longitudinal studies of violence—traps that we will soon discuss—pertain to studies of prosocial effects, and it would not be realistic to expect either clear answers or large effects. Nonetheless, effects may be larger than with violent programming, especially if one assumes that the child's everyday world more often models and rewards prosocial than antisocial behavior.

The Uses of Television for Health Promotion

In his foreword to *Television and Behavior*, Herbert Pardes, the Director of NIMH, wrote that the work "intends primarily to elucidate research findings and their implications for public health and future research." Thus, the hope is expressed that the report will spell out the implications of research for public health policy or practice.

The section of Volume 2 on public health contains introductory comments by Hamburg and Pierce outlining future research needs, which in their estimation largely revolve around the use of television as a major adjunct to other means of developing and enhancing healthful behavior. Next is an essay by Gerbner, Morgan, and Signorielli detailing the results of content analyses of the ways in which mental health, doctors, nutrition, safety, smoking, and alcohol are portrayed on television. Solomon then presents an essay on health campaigns in which he uses successful and less successful campaigns to illustrate the principles on which he thinks a successful campaign depends. Finally, Rubinstein and Sprafkin lament the dearth of knowledge about the roles that television plays with institutionalized patients, and they then present the results of a preliminary study on prosocial television for institutionalized children and adolescents.

Within the limits imposed by the sampling frame of only one week's dramatic content, Gerbner and his colleagues demonstrate some areas in which the reality of television distorts the reality beyond television. Among their claims are that portrayals of the mentally ill correspond more with stereotypes than with professional knowledge; that doctors are represented as omniscient and omnipotent; that children's shows and advertisements are a nutritionist's nightmare; that adult characters spend more time snacking than eating meals; that major characters are almost universally slim and attractive; that smoking is rare, while alcohol abounds and is often used to "solve" personal crises. The argument is that television distorts reality, has harmful effects, and fails to attain the prosocial ends it could (apparently) attain so easily.

The chapter on television portrayals of health does not deal with leverage. How can those who want the content of television changed to reflect healthier practices bring this about when (a) the broadcast industry has little internal incentive to produce or air such materials; (b) external pressures to portray healthier behavior are limited by the First Amendment, a docile FCC, a Congress many of whose members are unwilling to take on the networks, and a low level of organized citizen interest to reform television; (c) companies in the multibillion-dollar food industry want a competitive edge through advertising; and (d) the networks already monitor advertisers' claims about health products and probably feel they are sufficiently responsible about health matters? Indeed, the networks could probably point to the apparent low frequency of portrayals of smoking and claim how responsible they are. Interestingly enough, we do not know why so little smoking is shown, and if there has been any decline, we do not know why it has occurred. Studying the smoking issue with writers, producers, and others might give insight into industry practices and suggest leverage points for how alcohol might also be treated. After all, the networks have little to fear from the relevant industry, for hard liquor cannot now be advertised on television, though beer, wine, and other products of liquor-producing conglomerates can be.

The essay by Solomon puts forth, in the author's own terms "only one idea. It is time to stop asking . . . 'Do the mass media work in health?' and start asking 'How can we best make them work?' " He answers his own question: through adequate problem analysis (using basic knowledge of the health problem under attack), by careful media selection and use (based on knowledge in communication research and practice), by appropriate message design (derived from theories of attitude and behavior change), and by evaluation aimed at improving the campaign. But we must not be complacent about any of these answers. At any one time medical research is open to multiple interpretations. Note, for example, the recent changes in belief about the role that different types of cholesterol play in cardiac involvement. Communication research is also far from definitive on all issues. Do we know, for example, whether a media campaign will be effective by itself or whether it needs a face-to-face complement? The Stanford Heart Health Project, with which Solomon has been connected, is ambivalent on this count, for some data suggest the need for face-to-face services while other data suggest that the media will be just as effective without these services. As to behavior change, Bandura's social learning theory provides the currently preferred strategy in the three health promotion campaigns being funded by the National Heart, Lung, and Blood Institute and in the University of Pennsylvania project in Lycoming County, Pennsylvania. But this theory is more a framework of concepts to be considered in developing change campaigns than a blueprint for specific actions. It raises consciousness, but hardly details action plans.

More important than Solomon's suggestions are the issues he hardly raises, particularly about how community health promotion campaigns gain and hold their audiences. Solomon is most concerned with the question: "Given that we reach target audience members, how can we change them?" Thus, he considers the program "Feeling Good" a success despite the fact it went off the air because of poor ratings, even by the standards of PBS. The hope was that "Feeling Good" would re-create the success of "Sesame Street." It did not, and for important reasons that should give pause to anyone who wants to use television to promote practices deemed more healthy. Children of 4 are relatively more homogeneous than adults in their interests and background knowledge. They bring comparable cognitive structures to viewing and can mostly be motivated to view by a fast pace, kindly muppets, and cartoons. They probably also want to learn about letters and numbers, know little about these matters before age 4, and when they display their knowledge are sometimes rewarded by significant others. Contrast this with adults. They come from many different generations and have had very different levels of education and experience with health systems. Moreover, adults are (presumably) more motivated to watch television for entertainment than for health education, and few external rewards exist for gaining more health knowledge. If all, or even part, of this analysis is correct, public education about health matters is not likely to get over the first hurdle that commercial networks absolutely have to clear: the gaining and holding of a large audience.

The recent experimental health promotion campaigns do not rely on television alone. They are multimedia approaches, using television, radio, newsprint, and posters. Moreover, they have large community components involving physicians, nurses, and other public health workers as well as key local decision makers and local community organizations. Also, health screening is used to detect individuals at serious risk. Such an eclectic approach seems reasonable for gaining awareness, transmitting information, modeling recommended practices, and reinforcing compliance with these practices. However, it also reduces television's major advantage for public policy—its potential ability to reach large audiences at a low per person cost. In the large-scale demonstrations that Solomon analyzes, issues of cost-effectiveness loom large, not only in the sense of whether such campaigns should be preferred over sickness-oriented medicine, but also in the sense of whether television should be preferred over radio, print, etc. for those functions for which television is relevant.

If a decision were made that television is cost-effective in adding to a campaign, funds for televised health promotion would still have to be obtained. The broadcast industry is not likely to take upon itself the task of producing and airing such shows, because small audiences will be expected. Their interest would probably be limited to some talk show appearances, public service announcements, and regular paid advertisements. Some local stations may be persuaded to run low-cost specials, but it is not clear how many would do this or which time slots they would assign. We already have one cable channel that deals exclusively with health matters and that will soon reach most of the nation, and we may later have others. But who will support these channels financially, as advertises or subscribers? Some of the potential advertisers of a health channel have, after all, no desire to be associated with wellness. Sickness is their trade. Also, what will the profile of subscribers be like? Will it be biased toward the more or less affluent, the younger or older, the healthier or sicker? Will the need for finance mean that much of the programming is locally produced? If so, what will be its ability to capture and hold large audiences in light of the competition from other channels producing more expensive and more professional fare full of action and adventure?

Let us imagine that some production is funded from federal, state or local sources that also purchase air time for shows or advertisements that promote healthy practices. This should help improve the quality of programming over locally produced, low-budget shows. But how comfortable will public agencies feel with so much exposure and vulnerability to dissenting public voices about which behaviors are healthy and about how tax monies should be spent? Health promotion campaigns in which television plays a major role will be expensive and probably funded either out of the public purse or out of cable company set-asides for progamming that originates locally. The current broadcast industry has few incentives to be active in promoting public health. The NIMH report does not deal with this issue or with where the money might come from for future programming in the health promotion area.

The foregoing remarks suggest the need for a hard-headed analysis of television's role in health campaigns. Should it be used to advertise the telephone numbers of local prevention services, to raise awareness through appearances on talk shows and the local news, to broadcast well-advertised health specials, or to broadcast regular series on health? In most of these contexts, it seems to us more appropriate to talk about "Television and Its (Minor?) Role in Health Campaigns" rather than to talk, as Solomon does, about "Health Campaigns on Television."

Effects of Television's Distorted World

THE CULTIVATION HYPOTHESIS

Content analyses have shown that television presents a social reality which differs from the world in which we live. For instance, violence occurs on television atypically often, men outnumber women three to one, and minorities, blue-collar workers, young children, and the elderly are underrepresented (Gerbner et al., 1980). Gerbner and his colleagues have suggested that this television world reflects power relationships in U.S. society, with men and whites being dominant, and women, children, and blacks tending to be seen as victims. Gerbner and his associates have also maintained that exposure to this distorted world makes heavy viewers more sexist, anomic, fearful of crime, and negative in their stereotypes of the elderly. They further claim that this process of cultivating perceptions homogenizes society, reducing the degree of diversity that would pertain if there were no heavy viewing of television. The claims of Gerbner and his colleagues are frequently discussed in the NIMH report.

Several chapter authors note that, when multiple controls are simultaneously employed to control for third variable "causes" (cf. Hughes, 1980; Hirsch, 1980), some of the relationships published by Gerbner decrease to a level not different from zero. This has been demonstrated, for example, with the fear of walking alone at night, with other indicators of being a crime victim, and with the perceived fairness of bringing a child into the world. However, other relationships remain above zero even after multiple controls have been simultaneously employed. Thus, Tyler (1980) found that mass media presentations of crimes still influenced estimates of the general crime rate; Neville (1980) found that television exposure still influenced interpersonal mistrust; and Gerbner et al. (1980) discovered that viewing was still related to anomie among respondents with some college education.

A puzzle is presented by the finding that, after multiple controls have been instituted, viewing remains significantly related to some dependent variables but not others. Within the NIMH report, Comstock sought to solve the puzzle by postulating that heavy viewing cultivates pessimism about the world in general but does not increase one's personal fear of being a crime victim. Hawkins and Pingree concluded that television viewing increases interpersonal mistrust and estimates of the prevalence of crime but does not affect either personal fear of being victimized or alienation, defined as judgments about how the unjust world affects oneself. The preferred hypothesis seems to be that television affects general beliefs about how the world is but not particular fears of how it will affect oneself. The theoretical mechanism underlying this hypothesis is presumably that beliefs about how the world affects oneself depend more on actual life experiences and social networks than on television, whereas perceptions of how the world is in general depend more on vicarious experiences that extend one's knowledge beyond the necessarily restricted external world with which one has direct contact. This reconciliation of apparently conflicting findings begins the important task of specifying the conditions under which individuals base their opinions more on vicarious than on real experience. Also, it provides a contingency context for limiting Gerbner's general cultivation hypothesis, replacing the question "how does heavy viewing affect X" with "under which conditions does heavy viewing affect X."

However, we should not forget that, while the correlations between television exposure and general beliefs about the world remain above zero even after multiple simultaneous controls have been instituted, they are still extremely modest—mostly in the .10 to .20 range. Moreover, they can be interpreted as causal only if the assumption is accepted that the control variables fully model the true selection process whereby heavy and light viewers come to hold different worldviews for reasons having nothing to do with television. Not everyone will agree that the control variables typically used in research on Gerbner's hypothesis fully model the relevant selection differences. Some psychologists might postulate that withdrawn people watch television more often and see the world as more fearful for reasons that relate more to their earlier direct experiences in the world than to the television fare they have seen. Scholars favoring such a selection model would worry about the extent to which the typical control variables—age, gender, income and the like—function as sufficiently good proxies to remove statistically all the effects of withdrawal. Such scholars might well contend that adding a valid measure of psychological withdrawal to the analysis might make the modest partial correlations between television and general beliefs shrink even further, perhaps to zero. If it did, there would be no point trying to reconcile why multiple controls shrink some relationships to zero. Thus, the task Comstock and Hawkins and Pingree set themselves depends on assuming that past research has used credible selection models—an assumption not everyone would be willing to accept.

MAINSTREAMING

Gerbner has acknowledged the shrinkage of correlations that results when multiple controls are used simultaneously, and his response has been to modify his general hypothesis about the effects of heavy viewing. He now maintains that such viewing should distort perceptions of reality only with individuals belonging to social groups whose view of the world is discrepant from the mainstream presented on television. Heavy viewing can obviously not have the same effect on social groups that already subscribe to the mainstream. Gerbner's revised hypothesis specifies a statistical interaction between level of viewing and whether the respondent does or does not belong to a group that already believes in the television mainstream. A corollary is that there will be more variability in perceptions of the world among light viewers from a variety of different social groups than among heavy viewers from the same groups. It is in this last sense that Gerbner now claims that heavy television viewing is homogenizing society.

Numerous instances of mainstreaming are cited in the NIMH report, all of which depend on three inferences. First, content analysis is used to infer the nature of the television mainstream; second, surveys are used to infer the social groups which, in the aggregate, believe in the television mainstream; and third, a graph (or some equivalent) is presented which shows that heavy viewers from groups with perceptions generally discrepant from the television mainstream nonetheless perceive the world much like heavy and light viewers from groups with the same worldview as television. Thus, Gerbner and his colleagues (1980) report that (a) the television world is scary; (b) persons with less education tend to be more anomic and mistrustful than persons with more education; and (c) heavy viewers with high education rate the fearfulness of the world more like heavy viewers with little education than like lighter viewers with more education. They also note that (a) alcohol is frequently consumed on television; (b) surveys suggest that people in better health are more likely to drink than people in poorer health; and (c) the drinking habits of heavy viewers in poor health are more like those of heavy viewers in good health than like those of light viewers in poor health. Research since the NIMH report provides more evidence. Morgan (1982) (a) used content analyses to argue that the behavior of boys and girls is presented stereotypically on television; (b) he used surveys to infer that girls' beliefs about gender-appropriate behavior are less stereotyped than boys'; and (c) finally, he showed that girls who view heavily have more stereotyped beliefs than girls who view less heavily and that their beliefs are close to those of boys whatever the boys' viewing level. Gerbner et al. (1982) argued that television presents political views that are middle-of-the-road, and they went on to show that, in terms of anti-comunism and support for restrictions to free speech, liberals and conservatives who view heavily are more alike than liberals and conservatives who view less often. Within the NIMH report, Gerbner, Morgan, and Signorielli show (a) that television frequently portrays snacking and the eating of fattening foods; (b) that people from high-income groups are more concerned about their weight than people from lower-income groups; and (c) that heavy viewers with high income are less concerned about their weight than their lighter viewing counterparts and are just as unconcerned as low-income persons in general.

The number and variety of demonstrations of mainstreaming are impressive and clearly merit closer methodological scrutiny. To begin this process, we will use the last example above since it permits us to make concrete a number of general points. The first concerns the characterization of the television mainstream. While this may be clear in many instances, it may not be in others. For instance, Gerbner and his colleagues maintain that the television mainstream is characterized by snacking and a lack of concern with the health implications of food. Yet they also contend that the majority of television characters are slim—indeed, slimmer than most Americans. Are we to conclude from the presentations of eating on television that there is little concern with health; or are we to conclude from the slimness of heroes and heroines that eating well is a feature of the lifestyle of persons worth emulating? Standards need to be established and defended for inferring what the television mainstream is. It will not always be obvious.

Second, in analyses of mainstreaming it is typical to trichotomize viewing for each of the different groups examined. This would seem to unconfound viewing and group membership. But it need not, for within any viewing category the social groups can still differ in average viewing. To make this clear, imagine our example of eating in three income groups. Among those viewing television more than four hours per day, the lowest income group may have viewed five hours on the average, the middle income group five and one-half hours, while the highest income group may have viewed eight hours. If this were the case, one would have to consider whether the data in the NIMH report are due to a simple effect of viewing in the highest viewing category or to the crucial statistical interaction of viewing and group membership on which an inference of mainstreaming depends. Tests of mainstreaming should treat viewing as a continuous variable, taking care not to truncate prematurely when creating the heaviest viewing category. The current practice of creating this category by truncating at four or five hours of viewing per day may lead to a range within the category that is so wide that it masks group differences in viewing. Better would be truncation at more hours per day or perhaps a transformation of the untruncated viewing variable to make it more normally distributed.

A third problem concerns the correlates of viewing levels and group membership. In our example, there are probably many ways in which high-income people who view heavily differ from their counterparts who view less, some of which will be related to a lack of concern with weight. To give only one possibility, those who spend more time viewing might be less active socially and may therefore have less need to be concerned about their weight and general appearance. Third variable controls always need to be dealt with in analyses of mainstreaming. Controlling for these variables is particularly complicated because group variables correlated with television viewing and a concern with weight are potentially *both* confounds *and* stratification variables whose interaction with viewing constitutes a test of the mainstreaming hypothesis. Thus, if the frequency of social activities were correlated with a concern with weight, why should one test the mainstream hypothesis by stratifying on income rather than on the frequency of social activities? Should one not stratify on each variable, as well as on any other group variable that is related to a concern with weight? If one chooses to stratify only by income, as in the NIMH report, one should have a rationale for selecting this variable over others; but none is specified in the report. Research on mainstreaming has not yet developed explicit criteria for selecting one possible stratification variable over another and has ignored the possibility that most group correlates of the dependent variable can logically function both as tests of mainstreaming and as possibly spurious third variable causes.

A fourth problem concerns how to control for third variables once one has decided what the stratification and control variables will be. The few cases in the mainstreaming literature that have used multivariate controls have employed linear regression, whose appropriateness needs defending. This is because one way of formulating the mainstreaming hypothesis is that the regression of viewing on concern with weight should take one form for low-income persons and a different form for high-income persons. As a result, the overall regression of income on concern with weight may not always be linear. When multiple controls are used, the analysis should always be preceded by tests of the linearity assumption. If the tests indicate nonlinear regression, and linear adjustments are nonetheless made, spurious conclusions will probably result. They can also result if the assumption of linearity holds. This is because the control variables constitute a multivariate selection model that may be incomplete or inadequately operationalized. There is no way to escape the great difficulties inherent in choosing selection models in television research, particularly where the research is cross-sectional, as has been the case with studies of mainstreaming to date.

A fifth methodological problem concerns the falsifiability of mainstreaming. Hirsch (1981) has pointed to the relationship between mainstreaming and resonance, the latter being a process that Gerbner has hypothesized will cause group differences to increase among heavy viewers. This is the opposite to what mainstreaming brings about. Until there is an exact theoretical analysis of the conditions under which mainstreaming and resonance occur, it will not be possible to falsify either construct. For instance, had our example not shown a convergence of belief among heavy viewers from income groups with different aggregate levels of concern about weight, it would have been possible to argue that mainstreaming had occurred but that its effects had been masked by the countervailing effects of resonance.

The literature on mainstreaming is so new that all of the implicit assumptions of tests of the hypothesis have not yet been forced out and critically examined. However, enough preliminary data exist to convey a sense of excitement, albeit an excitement tempered by the need to probe unresolved methodological issues. What makes the mainstreaming hypothesis more credible than earlier, more global assertions about the effects of heavy viewing is that television's effects are now made contingent on the interaction of viewing and group membership. This means less widespread and thus more modest effects. For example, television is now thought to undermine a concern with eating and weight, not among all high viewers, but only among those who enjoy high incomes. The affected subgroups will sometimes be relatively small when expressed both as a percentage of all persons and as a percentage of all persons in the particular social group that does not agree with the television mainstream. Indeed, heavy viewers with high incomes probably represent a relatively small percentage of all high-income persons, and certainly a lower percentage of their group than viewers of lower-income groups. But such modest percentages need not always be the case. For instance, in Morgan's (1982) analysis of sex-role stereotyping, we suspect that the heavy-viewing girls represent no smaller a percentage of all girls than the heavy-viewing boys do of all boys. Nonetheless, Gerbner's new hypothesis about mainstreaming is more modest in its presumed scope of effects than was the earlier and less differentiated hypothesis about cultivation.

A final point needs to be made in trying to understand the scope of the mainstreaming hypothesis. If some heavy viewers are indeed moved toward television's distorted mainstream, this is not a mainstream in which "anything goes." Television does not move heavy viewers toward radical values or beliefs. Both the politics and the economics of the industry dictate that television keep to the middle ground, that it avoid confrontation with regulators and organized interest groups, and that it attract sponsors by presenting programming acceptable to a large heterogeneous audience. Fringe ideas and characters rarely appear on television, and when they do the context makes clear their social unacceptability. However, within these boundaries numerous leanings can be expressed, and researchers are only now begining to characterize them empirically. For example, Gerbner and his colleagues (1982) maintain that the television mainstream tilts toward the political right on issues of segregation, communism, and civil and personal rights, and that it tilts toward economic populism when federal spending is at issue. Subtle analyses are needed of any network leanings within the restricted range of the acceptable where the television mainstream operates. But none are detailed in the NIMH report, nor is this presented as an area for future research.

Television Violence

The NIMH report is more definite than the surgeon-general's in concluding that television violence causes aggression. Indeed, this conclusion is repeated in many chapters in both volumes. Although an "official" conclusion is clear, a chapter by Milavsky et al. (1982) offers a dissenting opinion based upon the authors' own research. In his overview to the chapters on violence, Rubinstein notes that the dissenting opinion of Milavsky et al. is based on a single study that is discrepant with most past findings and is likely to be debated. The last is certain, for the study is of the type called for in the surgeon-general's report. It is longitudinal, television exposure and aggression are carefully measured, and the data analysis is state of the art. Thus, readers of the NIMH report may be in a quandary: the report unambiguously and frequently concludes that television violence causes aggression; yet an apparently exemplary new study within the report fails to support this same conclusion. Let us now examine the evidence for each of these conclusions.

THE CONVENTIONAL SOCIAL SCIENCE WISDOM

The NIMH report contains a literature review of the violence issue by Huesmann that accurately reflects the opinion of many other scholars (e.g., Comstock, in press; Parke et al., 1977). Only a minor part of Huesmann's chapter deals with whether television violence increases aggression, for like most others he believes that the issue is closed. His belief rests on the consistency of past findings from laboratory and field experiments, cross-sectional surveys, and panel studies. To accept such consistency as a basis for believing that television causes aggression, one must logically accept (a) that the different methods do not share biases operating in the same direction; and (b) that the past studies using unbiased methods have been accurately reviewed. Huesmann does not examine these issues, to which we now turn.

It is obvious why laboratory experiments might exaggerate any link between television violence and aggression for normal children in everyday settings. Experiments have to produce aggression to discriminate between the outcomes of different treatments. Yet aggression is a relatively rare event. So, experiments are designed (a) to minimize internal inhibitions against aggression, (b) to minimize external cues sanctioning aggression, and (c) to maximize the clarity and intensity of short-term experimental treatments that have been deliberately chosen because they are likely to foster aggression. Although appropriate for discriminating between treatments, this strategy is not obviously relevant to regular television programming in the home, where internal cues against aggression may operate more powerfully, situational cues sanction aggression, and television aggressors are punished, usually by characters with prosocial qualities that are themselves presumably worthy of emulation. Also, the violent scenes on television are interspersed with many different types of activity, and viewers are free to watch intermittently or with low levels of involvement.

Positive bias is also likely in cross-sectional surveys where the amount of exposure to violence is correlated with aggression scores, holding constant background variables that are thought to correlate with viewing and agression. The adequacy of this approach depends on how completely selection differences between heavier and lighter viewers are modeled and how well the constructs in the model are measured. Usually, the selection models are primitive, involving demographic variables such as age, race, sex, etc., which are presumably proxies for the true (and unknown) psychological and social factors that cause differences in exposure to violence. Campbell and Boruch (1975) have argued that "underadjustment" is likely to occur in this situation so that, while some of the true selection difference will be removed, not all of it will. The synchronous correlation between viewing and aggression is positive, although usually small—in the .15 to .25 range. If Campbell and Boruch are correct, some of the background differences between heavier and lighter viewers will not have been removed, and estimates of the relationship between television and aggression will be somewhat inflated. Not everyone agrees with Campbell and Boruch. Cronbach et al. (1977) believe that overadjustment will sometimes occur, and that the adjustment can even be perfect, though we would never know when it was. Our point is *not* that underadjustment bias has occurred in all past cross-sectional studies of television and aggression; it is only that a plausible case can be made that the bias might have operated.

Huesmann mentions six reports of field experiments involving random assignment in nonlaboratory settings. Four were said to have yielded evidence of a positive relation between violence viewing and aggression, the exceptions being Feshbach and Singer (1971) and Wells (1973). However, the study by Loye et al. (1977) is also an exception since the data show that the group assigned to a television diet of high violence was no more likely to commit hurtful behaviors than the controls. Moreover, the experiment of Stein and Friedrich (1972) showed weak effects confined to a subgroup of initially more aggressive children and, according to Parke et al. (1977), this result failed to replicate (Sawin, 1973). Thus, four of the six studies are problematic. Of the two others, the work of Leyens et al. is also included as one of three experiments reported in Parke et al. (1977), which therefore emerges as the crucial report claiming to find effects of television violence. However, these field experiments deal with institutionalized children; regular television viewing was forbidden, possibly creating frustration; and in two studies the boys assigned to view aggressive films were initially more aggressive. In our view, the field experiments on television violence produce little consistent evidence of effects, despite claims to the contrary; and the instances where an effect is claimed involve populations that seem to be initially more aggressive.

It is widely agreed that the best method for studying television effects involves longitudinal panel studies. Unfortunately, the state of the analytic art has recently changed. The formerly advocated analysis based on cross-lagged pannel correlations is misleading when test-retest correlations differ between variables. Then, the less reliable measure will spuriously appear to be causal (Cook and Campbell, 1979; Rogosa, 1980). Television viewing is nearly always less stable than aggressiveness, as in the study by Lefkowitz et al. in the surgeon-general's report and in the original data that Huesmann includes in his chapter in the NIMH report. It may also be the case with Singer and Singer (1980), who used cross-lagged panel analysis. Thus, a bias may also have operated in past panel studies to produce a spurious relationship between television and aggression.

Like most summaries of television violence and its effects, the NIMH report is somewhat uncritical in its claim that the question has been answered and in its implication that the effect is widespread. The reliance on evidence from multiple methods is admirable—in the abstract. But the viability of such a strategy depends on the absence of biases operating in the same direction across all of the methods. In our opinion, such biases *may* have operated, although we have no direct evidence that they did or that they operated to produce totally spurious effects rather than merely inflating true ones. Yet no analyst in the NIMH report tackled these issues, which are certainly not handled in meta-analyses of the violence literature (Hearold, 1979). Until they are tackled and put to rest, doubts about the consequences of television violence will not be stilled, certainly not because of a committee decree or the frequently voiced consensus of a scholarly community. And until they are tackled, we will have to rely disproportionately on panel studies which involve state-of-the-art design, measurement, and data analysis. We examine two such studies below, beginning with Milavsky et al. (1982). To examine their work we rely, not on the summary in the NIMH report, but on their published book.

MILAVSKY, KESSLER, STIPP, AND RUBENS

Approximately 2,400 elementary students between 7 and 12 years of age were surveyed over a three-year period beginning in 1970, together with 800 boys between 12 and 16. The survey was repeated six times for the elementary sample, and five for the older sample. The interval between adjacent waves varied, ranging from 3 to 12 months for the elementary sample and 5 to 19 months for the older sample. Television exposure was measured with a program checklist. Students indicated how many episodes of each program they watched, and an estimate of total exposure over the preceding four weeks was obtained. A violence exposure measure was also derived by weighting programs according to adults' perceptions of the amount of violence in them. To measure aggression, younger children rated which of their classroom peers tried to hurt others by pushing or shoving, by hitting or punching, by saying mean things, or by lying to get others in trouble. The four items were summed so that each child received a score based on the percentage of classmates nominating him or her as aggressive. A self-report aggression measure was used with the teenage boys. Separate indices were formed of personal aggression (four items), aggression against a teacher (two items), and property aggression (one item on stealing and another on property damage). These items were assessed by noting how many times youngsters reported engaging in such an act during the last four weeks. A delinquency index of six items was also constructed. Teenagers indicated the last time they were badly beaten up, arrested, participated in a gang fight, stole a car, hurt someone to get his money, or purposely hurt someone with a knife. Responses were coded 0 or 1 depending on whether the event had occurred within the last three months and were then summed to create a delinquency scale.

The basic causal model can be expressed in equation form as:

$$A(t) = a + b^*A(t-1) + b^*TV(t-1) + u(t).$$

Stated verbally, aggression at one time is made a function of aggression and television viewing *at a prior time*. The earlier aggression variable, $A(t-1)$, functions to remove the simplest form of selection difference associated with the fact that some children are more aggressive than others at the time $(t-1)$ measurement takes place. The television variable tests whether viewing violence at an earier time adds to the predictability of later aggression once earlier aggression levels have been statistically controlled. Since aggression and television viewing were measured six times within the elementary sample, the model was used to evaluate each of the 15 possible wave pairs. Thus, when Waves I and VI are being contrasted, the model tests to what extent television viewing for four weeks in 1970 adds to the predictability of aggression in 1973. A major assumption of the model is that the prior aggression and television variables are measured without error. Since this assumption is manifestly false, Milavsky et al. estimated LISREL models for each wave pair in addition to the basic regression model.

Milavsky et al. concluded: (a) that the findings for boys and girls were quite similar; (b) that the lagged television variable was associated with many small positive coefficients, of which no more were statistically reliable than may be due to chance; and (c) that when socioeconomic status was included in the model as an additional control variable, the predominance of positive coefficients disappeared for elementary boys and the size of the coefficients decreased for elementary girls and teenage boys.

It is worthwhile displaying the major results from the LISREL analyses of children in the elementary sample with valid data. These are not much different from the basic regression results in the NIMH report. Table 1 shows that the estimates for the lagged television variable are small and rarely reliable. However, for both boys and girls the estimates are predominantly positive, suggesting that viewing television violence may have a small effect on aggression. The tendency is clearer for boys than girls. However, there is clearer evidence of the same effect with a large subset of girls. In the final column of Table 1 we also present basic model results for all the girls who attended schools classified as middle class and white. These constitute 70 percent of the total sample of girls, and are of special

Table 1. Results of LISREL Analyses by Milavsky et al. for Elementary Boys and Girls

Wave Pair	*Duration*	*N (Boys/Girls)*	*A (t-l) Beta (Boys/Girls)*	*TV (t-l) Beta (Boys/Girls)*	*TV (t-l) Beta*[a] *(Middle-Class Girls)*
I–II	5 mos.	364/391	.724/.749	−.046/−.090	−.004
I–III	9 mos.	356/384	.714/.691	−.018/−.024	.013
I–IV	1 yr.	349/369	.668/.682	.038/.008	.051
I–V	2 yrs.	211/236	.624/.496	.096/.078	.032
I–VI	3 yrs.	112/113	.571/.651	.160/−.024	.137
II–III	4 mos.	413/426	.876/.801	.046/.087*	.058
II–IV	7 mos.	409/408	.792/.676	.124*/.132*	.092*
II–V	1 yr./7 mos.	240/245	.694/.638	.014/−.006	.015
II–VI	2 yrs./7 mos.	121/123	.699/.601	.070/.213*	.144
III–IV	3 mos.	497/491	.885/.825	.078*/.068*	.041
III–V	1 yr./3 mos.	291/292	.753/.657	.015/−.043	.022
III–VI	2 yrs./3 mos.	147/133	.663/.610	.133*/.141	.200*
IV–V	1 yr.	301/296	.759/.513	−.043/.074	.088
IV–VI	2 yrs.	161/134	.721/.723	.059/−.022	.082
V–VI	1 yr	188/153	.750/.650	.075/.109	.109

* $p < .05$, all aggression coefficients are significant.

[a] Coefficients in this column are from OLS, not LISREL, analyses.

substantive importance because of the claim that controls for SES make the positive lagged coefficients shrink to zero. Yet for these middle-class girls the coefficients do not shrink to zero, despite the probable restriction in SES variance in the analysis.

A second point needs to be made about the results. Table 1 shows that the lagged coefficients representing the "effect" of television appear to increase with time, suggesting a larger effect the longer the delay between watching television and aggression being measured. The table is somewhat confusing because of instability in the estimates that we would presume to be greater the longer the delay interval. This is because attrition was greater with longer delays. The effect we are discussing can be seen more clearly in Figure 2, where the delay variable is expressed in six categories of equal width—a delay of 0–5, 6–11, 12–17, 18–23, 24–29, and 30–36 months—and where each data point represents the mean of all lagged coefficients falling within the time category. Figure 2 shows clearly that the coefficients tend to increase with time. The major exception is with the 18–23 month category, where the Wave V data are overrepresented. In nearly all our analyses, Wave V provided unusual data for reasons of which we are not sure.

Table 2 presents some of the results from the NIMH report on teenage boys. The analysis involved taking the data from adjacent waves and testing how viewing at the first wave was related to

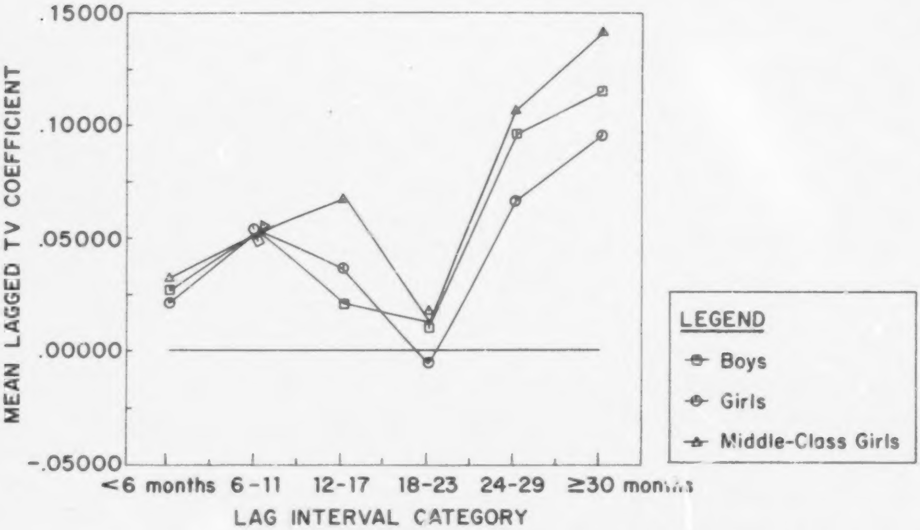


Figure 2. Relationship Between Lagged Television Coefficients and Delay Between Measurement Waves (Elementary Sample)

Table 2. **Results of the LISREL Analyses for Teenage Boys**

Pair	*Duration*	*A (t-1) Beta*	*TV (t-1) Beta*	*Mean TV (t-1) Beta*[a]
Personal aggression (N = 285)				
I–II	5 mos.	.530	−.023	.000
II–III	7 mos.	.443	.058	.034
III–IV	1 yr./7 mos.	.454	.094*	.058
Teacher aggression (N = 272)				
I–II	5 mos.	.546	.044	.090
II–III	7 mos.	.420	.036	.004
III–IV	1 yr./7 mos.	.236	.074	.030
Property aggression (N = 291)				
I–II	5 mos.	.418	−.059	.024
II–III	7 mos.	.320	.144*	.124
III–IV	1 yr./7 mos.	.408	−.025	−.044

Delinquency aggression (N = 172)[b]

Predicted probability of initiating delinquent behavior among teens with earlier television scores of . . .					*t-Ratio of b*
(5th percentile)	*(10th)*	*(50th)*	*(90th)*	*(95th)*	
.212	.229	.304	.436	.508	2.01*

* $p < .05$, all aggression coefficients are significant.

[a] Unweighted average over *all* wave pairs of identical duration. No statistical tests were conducted.

[b] This result is from a logit, not LISREL, analysis.

aggression at a later wave, holding constant *all* prior aggression waves, *all* previous television measures, and the relationship between the television measure whose causal influence is being assessed and the immediately preceding measure of aggression. Table 2 reports the lagged coefficients for the first three adjacent wave pairs, where the sample size is largest and the delay interval increases. Although there are two wave pairs separated by five months (I–II and IV–V) we report only the former, since it has not been subjected to as many controls removing the effects of prior television viewing. For the measure of personal aggression, which most closely corresponds with the measure used with elementary boys and girls, the results indicate that two of three lagged coefficients are positive and that their magnitude increases with the length of delay. For the measure of teacher aggression, the lagged coefficients are again positive but there is no evidence of greater magnitudes with longer delays. For the measure of stealing and damaging property, the results are inconsistent. Since these analyses use only part of the data, we in addition present the unweighted averaged lagged coefficients across all tests with the same lag period. The same patterns result for personal aggression (nonnegative coefficients that increase with delay), for teacher aggression (positive coefficients that are not related to delay), and for reported stealing and damaging property (inconsistent results).

It is difficult to interpret the results presented for the delinquency scale. Since acts of delinquency are rare and distributions are extremely skewed, Milavsky et al. conducted logit analyses for all wave pairs. However, they acknowledge that only three of the six items were asked at Wave I and that responses were disproportionately prone to error at that time. Hence, we assign little weight to the Wave I results. Milavsky et al. also acknowledge that the responses at Waves III and V could not be edited as they were at Waves II and IV. Also, disproportionately few adolescents seem to have reported new acts of aggression between Waves IV and V, entailing a particularly skewed distribution and correspondingly insensitive analysis of Wave V data. Assuming linear increases, our reanalysis of their Table 10.6 shows that, per month, about 33 new acts were reported between Waves I and II (probably because of the new delinquency items added at Wave II), and about 8 new acts were reported between Waves II and III as well as between Waves III and IV, whereas only 3 were reported per month between Waves IV and V. Since the basic analysis was of the percentage of adolescents *who committed any kind of delinquent act for the first time in the prior three months*, it seems that for analyses of Wave V there were only about 9 adolescents distributed across all the different levels of viewing television violence. For these reasons, we also assign little weight to the results using Wave V.The three wave comparisons that remain—between Waves II, III, and IV—all show positive relationships between the amount of viewing and subsequent delinquency, with only one being statistically reliable; but it covers the longest time period of 25 months and is shown at the bottom of Table 2. Milavsky et al. concluded from the equal number of positive and negative coefficients that viewing television violence was not related to subsequent delinquency. But this conclusion is based on all waves of data, and so it fails to use what is known about the lack of correspondence between Wave I and other measures, about wave differences in the quality of measurement, and about the particularly skewed margins at Wave V, where the results could change dramatically if a few cases were reclassified. We remain agnostic, therefore, about the relationship between television violence and subsequent delinquency.

Milavsky et al. noticed the plurality of positive over negative lagged coefficients in Table 1, and they probed whether they could make this relationship disappear. They found that when an indicator of family socioeconomic status was entered into their basic model, the plurality disappeared for boys. They also found that when indicators of gender and school socioeconomic status were included in an analysis of boys and girls combined, the positive coefficients disappeared and no evidence remained of larger coefficients with longer delay intervals. However, the authors analyzed many third variables, and no a priori rationale was offered for singling out SES. Moreover, in the most crucial analysis, SES had to be used as a school-level variable although aggression was measured at the individual level. Also, Table 1 showed that lagged television coefficients are consistently positive and increase with time for girls in middle-class schools. Thus, the stratification by SES has not made *all* lagged relationships between television and aggression disappear, although there is presumably still considerable variability in home SES within higher SES schools. Finally, we have to note that analyses comparable to those of the girls were not presented for boys using the school-level measure, and that the individual-level analyses of the boys' data are not convincing because more than half of the cases are missing, irrespective of whether household income or *mother's* employment status (blue collar, white collar nonprofessional, or professional!) is used as the control variable. In our opinion, the need exists to explore complex interactions involving gender, SES and television viewing, but we are not sure that the measures of SES available to Milavsky et al. are adequate to perform this task.

Delayed effects of television would be expected by anyone who assumes that viewing violence has its impact through the slow buildup of influences attributable to cumulative viewing. Milavsky et al. realized that their model might underestimate cumulative effects, and they addressed the issue directly. Their first step was to estimate a regression model in which aggression was the dependent variable and controls were instituted for *all* the previous levels of aggression and the most recent television score. Thus, when aggression at Wave IV was the dependent variable, aggression at Waves I, II, and III were the controls together with television viewing at Wave III. The next step was to estimate a "cumulative" model that included the same variables plus all prior television measures—i.e., those at Waves I and II. The final step was to test whether the multiple R-square was higher for the cumulative than for the comparison model. Since it was not, Milavsky et al. concluded that cumulative viewing does not increase aggression.

However, they tested whether previous measures of viewing violence increased the prediction of aggression *over and above the prediction attributable to viewing violence in the period immediately prior to the measure of aggression*. In our previous example, this would be equivalent to testing whether television viewing at Waves I and II adds to the prediction of aggression at Wave IV over and above the prediction already provided by television viewing at Wave III. This is not equivalent to testing whether the effect of *all* previous viewing is zero, which we interpret to be a more relevant definition of "cumulative viewing." Further, their cumulative model controls for all prior aggression measures and not just the initial one. This means that, if the earliest television measures affected subsequent aggression, these influences would be removed as part of the control for aggression. Thus, in a four-wave model where A = aggression and TV = viewing television violence, a control for all aggression prior to Wave IV would remove the effects of the causal path $TV_2 \rightarrow A_3 \rightarrow A_4$. Alternative cumulative models need to be postulated and tested that include: (1) all direct paths from prior TV measures to the dependent variable—i.e., from $TV_1 \rightarrow A_4$, $TV_2 \rightarrow A_4$, and $TV_3 \rightarrow A_4$; (2) all indirect paths from early TV to later aggression via intermediate aggression—i.e., from $TV_2 \rightarrow A_3 \rightarrow A_4$; (3) all indirect paths from earlier aggression to intermediate TV to later aggression—i.e., from $A_2 \rightarrow TV_3 \rightarrow A_4$; and (4) all paths from early to later TV measures—i.e., from $TV_1 \rightarrow TV_2 \rightarrow TV_3 \rightarrow A_4$. It will be difficult to test models that incorporate all the ways that television violence might affect aggression, and the analyst might be better served bracketing unknowable "true" effects rather than trying to estimate a single point that represents the cumulative effect of television. For the present, however, the crucial point is that the cumulative model of Milavsky et al. fails to include all the paths through which television violence might affect aggression and so may provide an underestimate of the "true" effect.

Another problem in the work of Milavsky et al. is the considerable departure from normality of the aggression dependent variables. The consequences of the skewed distributions should be considered in light of the sample sizes that are inevitably smaller with longer lags and more specific subgroups. Thus, for elementary boys the largest sample size is 497 when contrasting Waves III and IV, and the beta for the lagged TV variable is .078 ($p < .05$). In contrast, the sample size is only 112 when Waves I and VI are compared, and the beta, while higher (.160), is not reliably different from zero. Because of the grossly nonnormal distributions and small sample sizes for the longer lags that are of greatest relevance for a cumulative model, it is difficult to place confidence in the tests of statistical significance reported by Milavsky et al. and regularly used by them as a screening mechanism for deciding whether "effects" have occurred. Instead, we prefer to look to magnitude estimates and stable patterns of replication.

Anticipating the critique that their basic regression analysis was not sensitive enough to detect true effects at conventional levels of statistical significance, Milavsky et al. conducted a sensitivity analysis. They took five of the variables with the largest synchronous correlations with aggression and substituted these for the television variable in the basic lagged regression analysis for two-wave data. Since three of the five substitute measures resulted in consistent and reliable lagged associations with aggression, the authors concluded that their method was sensitive and that their failure to detect reliable effects of television violence was not due to the model's insensitivity with the data on hand. Unfortunately, the sensitivity analysis cannot carry the inferential burden attributed to it. First, the authors selected, a posteriori, variables with the highest cross-sectional correlation with aggression. Yet there are important theoretical and practical reasons for examining the lagged relationship of television violence to aggression. Second, Milavsky et al. tested whether *any* effects can be detected. This is less meaningful than testing whether effects of a predetermined magnitude can be detected.

Milavsky et al. foresaw every problem we have brought up and their foresight far outstrips conventional practice. However, we believe that their analysis nonetheless (a) failed to probe strong models of television's cumulative impact; (b) failed to probe the possibility of interactions between television viewing, gender, and SES levels; (c) did not fully probe the implications of consistently positive small coefficients and larger estimates with longer lags; and (d) relied excessively on tests of statistical significance that have questionable power because of badly skewed distributions and only moderate sample sizes for analyses involving longer time lags.

We are not inclined to claim that the study by Milavsky et al. "proves" any conclusion. Fortunately, a second panel study is available that tested a similar (but not identical) causal model. The work is by Huesmann et al. (1982), a version of which was published in the NIMH report. However, that version used an inadequate analysis based on cross-lagged panel correlations. The work we now review involved a better analysis.

HUESMANN, LAGERSPETZ, AND ERON

Each year for three years, Huesmann et al. surveyed elementary school children in grades 1–5, 758 from the U.S. and 220 from Finland. Program checklists were used to measure television viewing. Students indicated their favorite shows and rated their viewing frequency. These responses were summed and weighted according to the program's violence rating, as determined by two judges. A peer-rating measure of aggression was used, for which children were told to consider only behaviors in the current school year. Also, a four-item index was constructed of the child's self-rated similarity to characters who were described as engaging in specific aggressive behaviors. This was intended as a measure of identification with aggressive characters. The basic regression model was like Milavsky et al. in that the dependent variable was aggression in 1978, and aggression in 1976 was statistically controlled. However, the viewing measure was of one type of *cumulative* exposure to television violence (i.e., the sum of violence exposure for 1976 and 1977) rather than the single viewing term mostly preferred by Milavsky et al. Also Huesmann et al. added grade level to their model.

The results were similar to Milavsky et al. in direction, magnitude, and the low frequency of statistical significance. Thus, for American girls the standardized regression coefficient for the lagged television variable was .135 ($N = 221$, $p < .05$), while for U.S. boys it was .079 ($N = 191$), for Finnish girls it was .012 ($N = 85$), and for Finnish boys it was .110 ($N = 80$). However, Huesmann et al. chose to interpret the findings as evidence of an effect, whereas Milavsky et al. chose to interpret coefficients of comparable magnitude as evidence of no effect.

Huesmann et al. tested a second model with boys in which later aggression was studied with prior aggression being entered before a term formed from the product of viewing and the extent to which boys thought they were like aggressive television characters. The authors found that the interaction coefficients were reliably different from zero in both countries, and concluded that the link between television violence and subsequent aggression may be mediated through children identifying with violent fictional characters and not applying enough reality discount to what they watched. However, this conclusion depends on the interaction term not being correlated with the main effect of television. Since Huesmann et al. specifically concluded that a main effect was present, they also estimated a model in which prior television viewing was included. This reduced the interaction coefficient to a level that was no longer statistically reliable. Thus, the hypothesis that causal impact is mediated through identification with aggressive fictional characters remains promising but not proven for boys and untested for girls.

WHAT DO WE KNOW ABOUT TELEVISION VIOLENCE?

Our analysis of the past evidence for a causal link between television violence and aggression suggests that there is less consistency in the results than claimed and that consistent biases *may* have operated to inflate past estimates. Our reanalysis of the newer panel studies by Milavsky et al. and Huesmann et al. suggests that: (a) no effects of television violence emerge that are so large as to hit one between the eyes; (b) early measures of viewing television violence add to the predictability of later aggression over and above the predictability afforded by earlier measures of aggression; (c) these lagged effects are consistently positive, but not large, but (d) they are rarely statistically significant, although no reliable lagged negative effects have been reported.

The evidence indicates that a small association can regularly be found between viewing violence on television and later aggression when individual differences in aggression are controlled at one time. But is the association *causal*? If we were forced to render a judgment, it would be: Probably yes. Milavsky et al. conducted scores of subanalyses controlling for third variables that might be related to television viewing and later aggression over and above any relationship through earlier aggression. It seems that, of these, only SES analysis made the pattern of small positive coefficients disappear, and for reasons given earlier we are not impressed by the analyses of SES. Also, Leonard Eron has reported to us that entering an individual-level SES variable into the causal model of Huesmann et al. failed to make their lagged coefficients shrink to zero. Thus, the lagged relationships seem to have withstood all tests except one; and that failure is open to debate. Even so, this is not the same as withstanding tests based on third variables that theory indicates are spurious causes; and third variables can always be discovered of which no theoretician has yet conceived. Consequently, proof of causation is never possible, and judgments on this issue always have to be made. In our judgment, the lagged effects are probably causal.

This judgment is also influenced by the past literature. In particular, the randomized experiments in laboratory settings very strongly suggest that violence on the screen *can* cause aggression in normal populations, even though they do not speak directly to whether televised violence causes aggression in other settings. Further, the slowly growing file of case studies of imitative violence (summarized by Comstock, in press) indicate that events which are rare in real life have sometimes been committed soon after they were televised as part of a dramatic fictional show or a news account. This suggests that television violence *can* cause violence in nonlaboratory settings on the part of presumably abnormal people. Thus, there is strong evidence of causation in the wrong setting (the laboratory) with the right population (normal children), and in the right setting (outside of the laboratory) with the wrong population (abnormal adults). Such auxiliary evidence does not provide proof that the lagged associations obtained by Milavsky et al. and Huesmann et al. with the right population in the right setting represent causal connections; but it does provide circumstantial evidence of such connections.

WHAT DO THE SMALL LAGGED ASSOCIATIONS "MEAN"?

By conventional statistical criteria, these lagged associations account for trivial proportions of the explained variance in aggression even after corrections for unreliability. Also, most of the aggression measures are indistinctly related to behaviors that are obviously antisocial. Some tap into "boisterousness," others into "incivility," and very few tap directly into behaviors that we know lead to physical violence in the home or to problems with the law. Until intrepid (or foolhardy) econometricians undertake television research, we will not have grounded statements of the form "For each million hours of viewing television entertainment we have X murders, Y assaults, and Z cases of child abuse."

Unfortunately, there is no credible research on the extent to which the aggression measures typically used in television research predict to overt aggression and criminal behavior. Nonetheless, we are prepared to surmise that the relationship between viewing television violence and committing violent acts of the type universally abhorred in the U.S. will be even smaller than the already modest relationship between television violence and acts of boisterousness and incivility. It seems to us highly unlikely that television plays more than a minor role in directly producing violence and violent crime in the U.S. Certainly, its role is likely to be minimal when compared to many other social forces. Yet unlike these other causes of violence, television can be controlled technically, largely by limiting what is transmitted.

Although the effects of television may be small *on the average*, we do not yet know how the quality of interpersonal relations is changed when the general level of incivility among a large number of children is raised, whether by .5 percent, 1 percent or 2 percent. Also, the estimates of Milavsky et al. and Huessmann et al. represent an average, and a small number of the children who make up the average may suffer much larger consequences. Further, studies of television violence begin with children who have been watching television for several years, and they end well before the children have stopped watching violence. If effects increase in magnitude with more years of viewing or with longer delays between viewing and measuring effects, then the panel studies we examined in detail may have underestimated cumulative effects that can only be sensitively estimated from very large studies covering the two decades from infancy to late adolescence. All these limitations suggest that we have little basis for assessing the implications of the small lagged relationships between television and aggression observed over three years; and we cannot interpret them either as indicators of the need to reform entertainment programming or as indicators of the need to drop the violence issue as a dead end.

The NIMH report is too complacent, we believe, in its reasons for believing that television violence causes aggression and in its failure to describe the size of obtained effects. It is also inadvertently misleading in failing to discuss the implications that follow if the effects are considered "small" and if their relationships to physical violence or criminal behavior are unclear. Many readers understand "aggression" in terms of physical violence with intent to harm or as criminal behavior, and not as the "incivility" that the majority of past measures of aggression mostly tap into. Still, in interrelating the issues of prosocial and antisocial programming, the NIMH report does change in an important way the debate on television violence. We are now asked to measure the effects of such violence not only in terms of the aggression it might cause, but also in terms of the opportunities for positive growth that television violence precludes. For anyone who accepts this new frame for reference, the "small" direct effects of television violence inevitably understate the total direct and indirect costs of this type of programming.

Where Is the Leverage for Programming Changes?

It is difficult to predict how the NIMH report will be used. The theory of research utilization undergirding it seems to be the traditional theory of most basic researchers. It can be characterized as: Publish what we know to be the facts, and let them generate their own uses. The hope is that journalists, legislators, or judges will read the report and use its findings in ways that directly lead to programming changes, perhaps through regulation or creating public pressure on the networks. Another hope may be that individual parents will use the report to regulate their children's viewing; and another may be that future books on the media will cite the findings and influence how writers, producers, network executives, activists, and parents come to think about television.

The same theory of research utilization seems to be widespread among academic researchers of television. Most of them seem to care little about how their research is used. Their goal is a theoretical understanding of television and its roles in society. Other researchers care more about usage but are frustrated by the knowledge that academic research has rarely led to programming changes. The most salient exception to this generalization is offered by the surgeon-general's report of 1972, which led to the short-lived family viewing hour and may have contributed to what the CBS Office of Social

Research (1981) believes has been a small decrease in television portrayals of violence over the last 10 years. But the uses of the surgeon-general's report were not as dramatic or as longlasting as hoped, and may have been as much due to the short-lived pressure and publicity generated soon after the time of publication by Senator Pastore and the academic research establishment as to the quality of the evidence about the dangers of television violence.

It is not realistic to expect that the NIMH report will be used by the FCC to justify regulatory changes (Krattenmaker and Powe, 1978; Bollinger, 1982). This is partly because of the difficulty of establishing clear and present dangers from television. It is also partly because the FCC has been docile in the past and is financially dependent on a Congress whose members do not like to oppose the networks. Further, the courts have traditionally upheld the current dominant interpretations of the First Amendment that make government intervention into program decisions extremely difficult. The courts are also enjoined to seek the least harmful means of dealing with any negative effects of television. These are more likely to involve educating parents to control the selector knob better than decisions to modify program content, which would reek of censorship. The censorship issue is most salient with portrayals of real-life violence on the news. If television violence is harmful and should be regulated, surely this should apply to the news as well as entertainment programming, if the two can ever be clearly differentiated. For all these reasons, it is highly unlikely that the FCC will seek to regulate television content any more than it already does.

It is also difficult to affect change through influencing public and political opinion in ways that force the networks into "voluntary" change. Few Americans are so dissatisfied with the content of current television that they are willing to pressure legislators, to boycott certain advertised products, or to create events critical of programming that get extensive press coverage. Organized lobby groups are now trying to reduce the violence and sex on television or are attempting to reform children's advertising. But none of these poorly funded groups seem to us to be effective in significantly modifying the nature of television programming and advertising. They are indeed bothersome to the networks and may even be effective within the limits imposed by their size. Moreover, we do not know how television would be if they were not there as watchdogs. Nonetheless, their national impact does not seem large, and it is our impression that the networks treat them more like annoying gnats than strategically targeted missiles. Yet under different conditions the public interest groups could be more effective nationally, and we are not willing to assume that public pressure can never push the networks into "voluntary" change.

A third political source of leverage is through the broadcast industry's own attempts at self-improvement. The National Association of Broadcasters has standards which producers try to follow and which can be changed; the networks' own research staff studies the popularity of programs and the factors in them that make for popularity, and their findings can help change programs; the research staff also uses behavioral science consultants to comment on story plans in order to force out their potential for harm, and this process is attended by staff from departments concerned with standards. Finally, writers and producers have their own artistic standards, some of which can also change.

Little academic research speaks to the information needs of these groups within the broadcast industry, since the research deals with effects and not with ways to gain and hold audiences. There are some exceptions. For instance, Belson (1978) substituted the question of which types of violent programming cause aggression for the more traditional question of whether violence causes aggression, hoping to discover those forms of exciting violence that hold audiences but do not have harmful consequences. With a similar intent, Himmelweit et al. (1980) examined the nonviolent characteristics of programs that hold audiences, while in the NIMH report Comstock used laboratory experiments to detail the factors in violent programming that lead to less aggression. While useful in intention, these particular strategies have resulted in a limited payoff for industry self-improvement. To give only one example, Belson claimed that sports, cartoons, and slapstick comedy do not increase aggression, while realistic dramas and Westerns do. To ask writers and producers to eliminate or reduce realistic drama would be like asking them to refrain from exercising their craft on all interesting topics dealing with everyday life. More cogent would be research questions about the types of realistic violence that do and do not influence aggression.

In the more applied social sciences it is customary to ask, Where are the sources of leverage for change? and to follow this with the question, How can research best speak to the sources that have been identified? This approach involves different assumptions from those undergirding the model of use in the NIMH report and most academic research, where one begins with the assertion: This is what we know, and follows it with the question, How can this knowledge be used? In our opinion, the academic model has not yet produced much usage because it is not sensitive to where change can be effected. The knowledge is relevant to regulatory change, but such change is hardly possible given the stance of the FCC and the courts. It is also relevant to groups that want to reform television, but these groups are not now powerful or particularly effective. And while some kinds of knowledge might be used by the broadcast industry for self-improvement, the research community in universities is not producing information of this type. It is concentrating on effects of television and is ignoring the industry's desire to identify only the subset of programming changes that manifestly does not compromise audience size. Although academic research on television is somewhat successful in cataloging effects and beginning to understand why they come about, it is largely unsuccessful in using research to improve the entertainment content of television. One reason for this is that much television research is undertaken without first identifying where leverage for programming change exists and then tailoring the research to the pressure points identified.

Where, then, does leverage lie? Unfortunately, we are not as sure of an answer as we are of the importance of the question and of the need to explore it. In our opinion, the best place to start such an exploration is to go to Himmelweit's model and examine the broadcast industry (the networks, production companies, local stations, and advertising agencies) from a multidisciplinary perspective. This would stress the internal economics of the industry, its politics, sociology, and management, and it would also probe how the industry is linked to the institutions with which it interfaces (the FCC, Congress, high technology companies, university-based researchers, law firms representing the networks, activist groups, and various other public opinions). In such research the nature of schedules, shows, and advertisements would be dependent as well as independent variables; the predominant unit of analysis would be institutions rather than individuals; the methods would be historical and qualitative as well as quantitative; the causal models would involve reciprocal as well as unidirectional causal influences; and a special emphasis would be placed on the leverage points already identified so as to explore the conditions under which regulation, public opinion, and internal standards can help bring about changes in programming.

Such research would do more than fill out some of the glaring blanks in Himmelweit's model. It might also lead to information about specific sources of leverage of which we are not now aware and to which research could speak. We cannot be certain that research on the institutions related to television will eventually provide a theory of leverage for programming change. But we are confident that such research cannot influence programming less than the future research plans outlined in the NIMH report. Basically, these call for more of the same, but with a somewhat greater emphasis on new technology, on how adults as well as children are affected by television, on prosocial rather than antisocial effects, and on causal explanatory models instead of simple bivariate causal relationships. Since the NIMH priorities for the future overlap heavily with the research emphases of current television research, we feel all the more obliged to suggest the need for an alien perspective on the report's assumptions that may help alleviate the current impotence of television research (a) through study at the institutional rather than the individual level, and (b) through an explicit search for knowledge of leverage points that may affect programming changes. We have few illusions about how easy such research will be, particularly because the broadcast industry is private, is suspicious of the implicit elitist values of most researchers, and has few incentives to be cooperative with researchers. Nonetheless, much can be done with persistence, the creative use of public documents, and with access to individuals who have firsthand knowledge of the industry because they are or have been in it.

Conclusion

We are convinced, in general, that the NIMH report presents a reasonable summary of current knowledge about television, its effects and its potential. Our reservations touch upon a lack of realism in dealing with how the potential for prosocial effects can be realized; a lack of concern with the generally modest, if not small, negative effects of television; and an exaggerated sense of the confidence we should place in many past findings. The report convincingly takes us beyond the "no effects" era of Klapper (1960), when television was held mostly to reinforce already established beliefs and behaviors; but it does not convincingly take us as far as the authors of the report want to go in portraying television as a source of negative effects of immediate and pressing concern and as a force that could easily be harnessed for more prosocial ends. Evidence of negative effects is apparent, as is evidence of the potential for prosocial accomplishments. What is missing in the NIMH report, as in nearly all television research, are mechanisms for going from the evidence produced by television researchers to changes in television practice. We would be happy indeed if this article helped create a debate about whether such mechanisms are important, what they might be, and how they might be researched. Personally, we are apprehensive about the good to be gained from more television research of the dominant type conducted in the past and advocated for the future in the NIMH report. Such research produces descriptive findings, and helps begin the process of causal understanding. But since this type of television research places university-based scholars in a relationship of antagonism or irrelevance to those responsible for television practice, it needs supplementing with approaches that have a greater likelihood of directly and indirectly shaping the content of entertainment programs.

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John Hinckley, Jr. and the Insanity Defense: The Public's Verdict

VALERIE P. HANS AND DAN SLATER

On June 21, 1982, a Washington, D.C. jury found John W. Hinckley, Jr. Not Guilty by Reason of Insanity (NGRI) on all charges arising from his attempted assassination of President Reagan. No verdict in recent history has evoked so much public indignation. An ABC News poll conducted the day following the verdict revealed that three-quarters of the Americans surveyed felt "justice had not been done" in the Hinckley case (ABC News, 1982b). Interviews with members of the public (e.g., Phil Donahue Show, August 9, 1982; ABC News, 1982a, 1982b) as well as letters to Judge Barrington Parker, who presided at the trial (New York Times, 1982b), have documented the public's negative opinion of the verdict.

The public's negative reaction has stimulated reforms of the insanity defense. The day after the Hinckley verdict was reached, the

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Abstract Public furor over the Not Guilty by Reason of Insanity verdict in the trial of John Hinckley, Jr. already has stimulated legal changes in the insanity defense. This study documents more systematically the dimensions of negative public opinion concerning the Hinckley verdict. A survey of Delaware residents shortly after the trial's conclusion indicated that the verdict was perceived as unfair, Hinckley was viewed as not insane, the psychiatrists' testimony at the trial was not trusted, and the vast majority thought that the insanity defense was a loophole. However, survey respondents were unable to define the legal test for insanity and thought Hinckley would be confined only a short period of time, contrary to the estimates of experts. These findings, in conjunction with other research showing the public is not well informed about the insanity defense, underscore the importance of examining determinants of opinion about the insanity defense before additional reform is undertaken.

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Delaware legislature passed a law providing a Guilty But Mentally Ill verdict alternative in insanity cases. Proposals to abolish or restrict the insanity verdict are before legislatures in other states, and the White House has announced its plan for revision of the insanity defense (Hoffman, 1982; Philadelphia Inquirer, 1982; Putzel, 1982; New York Times, 1982a). The Hinckley trial promises to be a benchmark in the reform of the insanity defense.

The purpose of the study reported here is to provide a more complete account of public opinion about the Hinckley trial and insanity defense. In an attempt to understand the determinants of reactions to the trial, the paper also explores demographic and attitudinal correlates of opinions about the Hinckley verdict.

Previous research on perceptions of the insanity defense is sparse, but the available literature indicates that the public always has taken a dim view of the defense (e.g., Moran, 1981). Public opinion polls consistently have shown that a majority of Americans believe the insanity defense is a loophole that allows too many guilty people to go free (Bronson, 1970; Fitzgerald and Ellsworth, 1980; Harris, 1971). Perhaps as a consequence of this perception, the reluctance of juries to find defendants NGRI is legendary. More detailed analyses of perceptions of the insanity defense or the criminally insane show decidedly negative attitudes but widespread misconceptions (Howell, 1982; Pasewark, 1981; Steadman and Cocozza, 1977).

Therefore, we expected our survey to reveal: (1) considerable negativity about the insanity defense in general, and the Hinckley verdict in particular, and (2) poor to moderate knowledge about the insanity defense.

Method

SURVEY RESPONDENTS

Respondents were 434 men and women from New Castle County, Delaware.[1] Respondents were contacted by telephone using random-digit-dialing techniques. The telephone numbers were selected by computer using a random number generator from the universe of all telephone numbers of all prefixes in New Castle County. The demographic characteristics of the sample were as follows: 41 percent male and 59 percent female; 86 percent white, 12 percent black, 2 percent other nonwhite; 5 percent were less than 18 years of age; 11 percent were 18–24 years; 29 percent were 25–34 years; 17 percent were 35–44 years; 17 percent were 45–54 years; 12 percent were 55–64 years; and 10 percent of the sample was 65 or more years of age. A third of the sample (34 percent) had only a high school diploma, while 14 percent had not graduated from high school; 27 percent had completed one to three years of college, 15 percent had a college degree, and 10 percent of the sample had done graduate work.[2]

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[1] New Castle County encompasses the northern third of the state of Delaware and includes the urban center of Wilmington, the university community of Newark, and both suburban and rural/farming areas. New Castle County is the home of major corporations (e.g., DuPont, Hercules) as well as large automotive plants (Chrysler, General Motors). Census studies of voter registration, persons voting, and households with telephone service show that residents are within 3 percent of national averages. Delaware residents, on the whole, have a higher per capita income than the national average (Bureau of the Census, 1980).

PROCEDURE

Nine paid interviewers conducted the survey beginning one week following the announcement of the verdict on four consecutive evenings from June 28 to July 1, 1982. The interviewers included one faculty member, four graduate students, three university graduates, and a college senior. All but one had previous experience with telephone interviewing or had systematic training in survey research techniques prior to this study. Interviewers went through a group training session for this study with the project director. Topics covered in the training session included: (1) use of random telephone number lists; (2) introductory statement to the respondent; (3) question-asking techniques; (4) the range of possible responses; (5) dealing with the open-ended questions; (6) prompting methods; and (7) recording the data on the questionnaires. The project director monitored calls by all of the interviewers to ensure standardization of data collection methods.

The interviewers introduced themselves to the person answering the telephone by saying:

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[2] Demographic data for Delaware collected in 1980 by the United States Census Bureau allowed us to estimate the representativeness of our sample. The racial composition of the community (86 percent white, 14 percent nonwhite) was reflected accurately in our sample, but women were overrepresented in the sample (59 percent) as compared to the census (52 percent). Census data indicated that 26 percent of New Castle County residents who were 18 years of age or older had less than four years of high school, and 19 percent had one to three years of college education. Our sample (which included some respondents under the age of 18) had relatively fewer respondents without a high school diploma (14 percent) and relatively more with one to three years of college (27 percent). Finally, comparison of age categories indicated that our research project undersampled individuals under the age of 24 (persons age 18–24 comprised 20 percent of the census but 11 percent of the sample) and oversampled people in the 25–34 age range (22 percent of the census versus 29 percent of the sample). Our sample thus was somewhat better educated and older than the community.

Hello. My name is ____. I am calling from the Department of Communication at the University of Delaware. We're conducting a brief opinion poll and would very much like your assistance. As you may have heard, John Hinckley, the man charged with attempting to assassinate President Reagan last year, has been on trial. He pleaded Not Guilty by Reason of Insanity, and the jury, in fact, found Mr. Hinckley Not Guilty by Reason of Insanity. We would like to know what you think about this case and would like to ask you a few questions.

Not all interviewers kept accurate records of refusals (some recorded business, nonresidential, and other inappropriate numbers with the same notation used for refusals), but the remainder of the interviewers had a refusal rate of about 5 percent. Another telephone survey of New Castle County residents also conducted in the summer of 1982 had a refusal rate of 3 percent (Ratledge, 1982). If there was no answer, or the number was disconnected or was a nonresidential number, interviewers proceeded to the next randomly generated telephone number. After ascertaining the individuals' willingness to participate, the interviewers proceeded with the questions.

QUESTIONNAIRE

The questionnaire contained items related to the Hinckley trial, the insanity defense, media use, attitudes toward psychiatry and the death penalty, and demographic characteristics (for question wording see Appendix). Since most questions had fixed answer choices, coding was relatively straightforward. The exception was the open-ended question in which respondents provided definitions of legal insanity. A coding system with 14 different categories was developed. Two raters coded each of the 434 responses individually and initially agreed on 86 percent of the answers. The responses which generated disagreement were discussed and in all instances a code was agreed upon by the two raters. In those few instances in which respondents provided multiple answers, raters selected the first coherent or complete statement for coding.

Results

REACTIONS TO THE HINCKLEY VERDICT

In line with expectations, respondents displayed very negative reactions to Hinckley's NGRI verdict, as shown in Table 1. About half thought the verdict was not at all fair, and another quarter felt it only slightly fair. When asked what decision they would have reached had they been jurors, the vast majority would have convicted Hinckley; only a minority would have reached the same NGRI verdict

Table 1. Reactions to the Hinckley Trial

Fairness of verdict	Not at all fair	53.7%			Slightly fair	22.4%
	Somewhat fair	15.4%			Very fair	8.5%
Respondent's verdict	Guilty	73.3%	Don't know	12.0%	NGRI	14.7%
Hinckley insane	No	65.7%	Don't know	10.0%	Yes	24.4%
Consequences of NGRI	Jail					2.3%
	Mental hospital					80.9%
	Go free					10.1%
	Don't know					1.8%
	Other					4.9%
Length of confinement	6 months or less					21.9%
	7 to 12 months					22.5%
	13 to 24 months					17.5%
	25 to 60 months					17.5%
	Over 5 years but less than life					13.5%
	Life					7.0%
What should happen to Hinckley	Punishment					26.4%
	Both punishment & treatment					59.5%
	Treatment					14.1%
Insanity defense is a loophole	Agree					87.1%
	Neither agree nor disagree					6.1%
	Disagree					6.8%
Confidence in psychiatric testimony	Not at all confident					39.9%
	Slightly confident					19.6%
	Somewhat confident					28.1%
	Very confident					12.5%
Psychiatrists' ability to determine legal insanity	All of the time					1.0%
	Most of the time					17.1%
	Some of the time					47.0%
	Every once in awhile					23.5%
	Never					11.4%
Definition of legal insanity	Complete definition					.2%
	Partly correct definition					29.0%
	Incorrect definition or don't know					70.8%

the jury rendered. A substantial majority believed Hinckley was not legally insane at the time of the shooting.

CONSEQUENCES OF HINCKLEY'S NGRI VERDICT

As indicated in Table 1, 80.9 percent of the sample correctly responded that Hinckley would be sent to a mental hospital as a result of this NGRI verdict. Some persons erroneously believed that Hinckley would either go free or be sent to jail. Of those respondents expressing an opinion on how long Hinckley would be confined, most predicted a relatively short period of confinement. A total of 61.9 percent said that Hinckley would be out in two years or less; only 20.5 percent thought Hinckley would spend more than five years in confinement. About a quarter of the sample felt that Hinckley should only be punished for his behavior, while a smaller percentage felt he should just receive psychiatric treatment. The majority felt Hinckley should receive both punishment and psychiatric treatment.

KNOWLEDGE AND ATTITUDES ABOUT THE INSANITY DEFENSE

Table 1 illustrates the sample's strong negative feelings about the insanity defense. An overwhelming majority (87.1 percent of the sample) agreed that the insanity defense is a loophole that allows too many guilty people to go free, while only 6.8 percent disagreed. Similar negative attitudes were expressed about the value of psychiatric testimony in legal insanity defenses. Over half the sample responded that they would have been "not at all" or only "slightly" confident in the psychiatric testimony had they been jurors in the Hinckley case. Respondents also revealed only modest confidence when asked to what extent psychiatrists could determine whether or not someone was legally insane.

The question asking for people's definitions of legal insanity indicated very little knowledge of the elements of the test for legal insanity. Only one of our 434 respondents gave a reasonably good approximation of the Model Penal Code definition of legal insanity which was used in the Hinckley case and was employed here in Delaware at the time of the Hinckley trial. In addition to the lone correct respondent, a little less than a third of the sample was able to provide one of the three elements of the test for legal insanity.[3]

PREDICTORS OF PUBLIC ATTITUDES TOWARD THE HINCKLEY CASE

To explore further the determinants of public opinion concerning Hinckley, multiple regression analyses using dummy variable coding as appropriate were performed on three variables which were thought to represent different aspects of reactions to the trial: the fairness of the verdict, the estimated length of Hinckley's confinement, and punishment-treatment orientation to Hinckley. The results of these analyses are presented in Table 2.

[3] There are three elements in the definition of insanity under the Model Penal Code test. Defendants are entitled to be found NGRI (1) if as a result of mental disease or defect, (2) they lacked substantial capacity to appreciate the wrongfulness of their conduct, or (3) were unable to conform their conduct to the requirements of the law. Among respondents who were able to provide at least one of the elements, 3.9 percent said legal insanity was "mental disease;" 12.3 percent answered that a person is legally insane when they "cannot tell right from wrong;" and 12.8 percent equated legal insanity with "having no control." The modal response produced by 23 percent of the respondents was "didn't know what he was doing." No other content code accounted for more than 6 percent of the responses.

Table 2. Multiple Regression Analyses of Reactions to the Hinckley Trial

	Fairness of Verdict		*Length Hinckley Confined*		*Punishment-Treatment*	
	Beta	F-*ratio*	*Beta*	F-*ratio*	*Beta*	F-*ratio*
Hinckley trial: knowledge & attitudes						
Fairness of verdict	—	—	.14	4.07*	.20	9.90*
Respondent's verdict	.35	46.61*	.01	0.03	.11	3.34
Confidence in psychiatric testimony at Hinckley trial	.13	7.01*	.09	1.93	.06	1.25
Hinckley insane	.13	6.29*	.04	0.31	.10	3.06
Consequences of NGRI	−.07	2.39	−.16	8.14*	.02	0.21
Length of confinement	.09	4.07*	—	—	−.05	1.01
Punishment-treatment	.16	9.90*	−.06	1.01	—	—
General knowledge & attitudes						
Psychiatrists' ability to determine legal insanity	−.01	0.07	−.06	0.98	−.20	12.82*
Definition of legal insanity	−.03	0.45	.09	2.50	.09	3.25
Insanity defense is a loophole	.12	6.24*	.09	2.34	.07	1.53
Death penalty scruples	−.00	0.00	.08	1.77	−.00	0.00
Death penalty verdict	−.04	0.55	−.01	0.01	.08	1.99
Demographic & media use variables						
Gender	−.03	0.38	.07	1.43	−.04	0.52
Race	−.01	0.02	.02	0.16	−.08	2.43
Age	−.09	3.67	−.03	0.28	.16	8.55*
Education	−.01	0.04	.06	0.91	.03	0.35
TV as main information source	−.05	0.60	.17	5.16*	.02	0.08
TV newscasts per week	.01	0.03	−.00	0.01	−.07	1.81
TV hours per day	−.00	0.00	.01	0.01	−.04	0.69
Newspapers as main information source	−.08	1.71	.12	2.52	.04	0.30
Newspaper days per week	.05	1.29	−.06	1.23	−.06	1.40
Multiple R^2	.43	11.71*	.15	2.67*	.29	6.23*

* $p < .05$.

NOTE: The degrees of freedom for the multiple regression analyses are 20 and 306, while the degrees of freedom for F-ratios associated with individual variables are 1 and 306.

Respondents' perceptions of the fairness of the Hinckley verdict were strongly related to the verdict they would have reached in the case. Other case-specific attitudinal variables also were significantly related to perceived fairness. Individuals who thought the verdict was fairer had more confidence in the psychiatric testimony in the trial, were likelier to consider Hinckley insane, thought he would be confined for a longer period of time, and favored treatment rather than punishment for him. They also were likelier to disagree with the statement that the insanity defense was a loophole.

For respondents' estimates of the length of time Hinckley would be confined, both perceived fairness of the verdict and electronic media as the primary sources of information about the trial were statistically significant predictors. People who reported that television or radio was their main source of information about the trial estimated that Hinckley would spend a longer period of time in confinement. However, other media use variables were not related to estimates of confinement length. Finally, there was a significant relationship between respondents' perceptions of the NGRI verdict consequences and their estimates of the time Hinckley would be confined. More severe perceived consequences (such as jail) were associated with greater length of confinement estimates.

A final dimension of public reaction to Hinckley is represented by the punishment-treatment orientation to him. The fairer the perception of the Hinckley verdict, the more likely respondents were to have a treatment orientation toward Hinckley. In addition, the perception of psychiatrists' abilities to determine when someone is legally insane was a significant predictor of punishment-treatment orientation. The more trust people had in psychiatrists' abilities to detect legal insanity, the more willing they were to prefer treatment for him. Age was a significant predictor; older respondents were more likely to prefer treatment over punishment.

Discussion

This survey of Delaware residents documents the negative reaction of the NGRI outcome of the Hinckley trial. Our sample respondents thought the verdict was unfair, believed Hinckley was not insane, had little faith in the psychiatric testimony presented at the trial, and asserted they would have reached a guilty verdict had they been jurors in the trial.

Despite intense media coverage of the Hinckley case, knowledge of the insanity defense was not extensive, supporting previous research showing that the public is not well informed about the insanity defense (Howell, 1982; Pasewark, 1981). The majority of our sample estimated Hinckley would be confined only a relatively short period of time. Of course, there is no way of determining precisely when, or if, he will be released, but experts indicate that Hinckley is likely to undergo a lengthy period of confinement (ABC News, 1982a). Since the trial's conclusion, Hinckley's behavior in the hospital (in threatening to rape and kill Jodie Foster) has if anything reduced the likelihood of an early release.

While the sample's attitudes toward Hinckley and the insanity defense are predominantly negative, the results evidence some ambivalence. The sample's strong but somewhat contradictory preference for a combination of punishment and treatment for Hinckley is at odds with the premise of our legal system that defendants should either be treated if legally insane or punished if legally guilty. While most of the sample thought Hinckley was not insane, most felt he should receive some treatment. Even among those respondents who said they would have found Hinckley NGRI, over half (54.9 percent) thought some punishment was appropriate for him.

The multiple regression analyses suggest that, along with other factors, people's views of the extensiveness, success, and consequences of the insanity defense affected their perception of the fairness of the Hinckley verdict. The impact of views about psychiatric testimony on perceptions of verdict fairness found in this survey is congruent with one study showing a relationship between attitudes toward psychiatry and opinions about the insanity defense (Arafat and McCahery, 1973). However, in contrast to Simon's (1967) research on jurors' verdicts in insanity trial simulations, demographic variables were largely irrelevant in this study.

As a sample of the residents of one geographical area, this study has obvious limitations in its generalizability. Coverage of the trial in this area was primarily by network television and radio news and national wire services, so that information about the trial in this community was probably similar to coverage in other areas. The overall negative reactions to the verdict and to the insanity defense found in this survey are, in fact, consistent with results of other polls (ABC News, 1982b; Bronson, 1970; Fitzgerald and Ellsworth, 1980; Harris, 1971). However, Delaware residents are comparatively better educated and somewhat more affluent than the national averages, characteristics which could enhance knowledge of and attitudes toward the insanity defense. Furthermore, our survey oversampled women, more highly educated individuals, and people 25–34 years old. While our data showed no strong or consistent effects for demographic variables, we cannot rule out their potential impact on attitudes toward the insanity defense.

The value of this study lies in its analysis of one community's reaction to this important trial and in revealing the complexity of the public's attitude toward the insanity defense. Two avenues for future research are suggested: (1) an exploration of the foundation of attitudes toward the insanity defense, in particular the apparent divergence between public opinion and contemporary legal rules governing the disposition of NGRI defendants, and (2) the accuracy of the public's knowledge about the insanity plea. Since legislators and policy makers have reacted to negative public opinion following the Hinckley trial by initiating reforms of the insanity defense, such research is essential.

Appendix: Questionnaire Items

1. Over the course of the trial, where would you say you got most of your information about the trial—from TV? Radio? Newspapers? Magazines? Friends? Relatives?
2. In your opinion, how fair would you say the verdict in this case was, on a scale of 1 to 5, where 1 is not at all fair and 5 is extremely fair?
3. If you had been a juror in the Hinckley case, what decision would you have reached—would you have found him guilty, not guilty, or not guilty by reason of insanity?
4. Again, if you had been a juror, how confident would you have been in the psychiatrists' testimony—on a scale of 1 to 5 where 1 is not at all confident and 5 is extremely confident?
5. To what extent do you think psychiatrists can determine whether someone is legally insane—do you think they can determine that all of the time, most of the time, some of the time, every once in a while, or never?
6. Now that John Hinckley has been found not guilty by reason of insanity, what do you think will happen to him? Will he go to jail, a mental hospital, or what? (If answer is jail or mental hospital: How long do you think he will be confined?)
7. What do you think should happen to John Hinckley—should he be punished, treated, or both?
8. In a few words, what do you think is the legal definition of insanity?
9. According to your definition, and from what you know and have read and heard, do you think that John Hinckley was legally insane at the time he shot President Reagan?
10. How strongly do you agree or disagree with the following statement: "The insanity defense is a loophole that allows too many guilty people to go free." Do you strongly agree, agree, disagree, strongly disagree, or neither agree nor disagree with that statement?

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In Search of a Constituency for the "New Religious Right"

CAROL MUELLER

JERRY FALWELL AND BETTY FRIEDAN agree on at least one thing. Both believe that the issues articulated by the movement for women's liberation were decisive in the last presidential election. Whereas Friedan (1981) argues that the Democrats lost because they paid scarce heed to women's issues, Falwell believes that he and other leaders of the Moral Majority and the "New Religious Right" mobilized a winning constitutency through their opposition to abortion, sexual preference, the Equal Rights Amendment, and women's new roles outside the home as wage earners and office holders. Of course, other issues concern the Moral Majority as well—television programming and the teaching of evolution or sex education in the

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Abstract The paper evaluates claims by the "New Religious Right" to have created a mass constituency or a new "moral majority." The paper examines evidence for the development over the 1970s of new consciousness which reflected politicization of a growing moral opposition to three feminist issues: abortion, sexual preference, and women's liberation. Comparisons are drawn with attitudes toward 11 national spending priorities. All the findings run contrary to claims for a newly politicized "moral majority." The basic findings are these: (1) there is no conservative trend on the three feminist issues for 1972–80; (2) factor analytic techniques reveal simple factor structure within the three issues but not between the three issues over the decade; (3) politicization of the issues is greater in 1974 than in 1977; and (4) the differences between the religiously involved and others have existed since the beginning of the decade and show no evidence of increasing politicization during the 1970s. Data are from the NORC General Social Surveys, 1972–80.

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public schools. It is also true that the Moral Majority represents only the more religiously oriented arm of the New Right, which also includes and partially overlaps with Fiscal Conservatives and Anti-Communists (Crawford, 1980). Yet, despite these qualifications, it is only the Moral Majority which, in 1980. claimed a totally new constituency of voters.

The present study asks what credence we should give this claim. Did the Moral Majority create a new consciousness which could be mobilized in the congressional races of the late 1970s and the presidential election of 1980 to serve the political ends of the New Right? This paper explores one set of empirical issues based on this question. The focus here is on the issues arising from the movement for women's liberation and whether there is evidence to support the Moral Majority claim that a new constituency was mobilized in opposition to these issues during the 1970s.

Mode of Analysis

The strategy employed in the analysis is based on the assumption that political mobilization is accompanied by cognitive restructuring.[1] There is strong evidence from both political science and sociology indicating that the belief systems of political activists are, in Converse's (1964) terms, more highly constrained or consistent than those of nonactivists. In the present paper, I draw on the belief system tradition of research to develop criteria for identifying the presence of belief restructuring among the general public. The analysis proceeds in four steps.

The first step is to look for conservative trends over the 1970s in responses to questions on four different issues. Three of these include sets of items tapping those "single issues" which achieved prominence with the rise of the movement for women's liberation—abortion, sexual preference, and women's roles outside the home.[2] These are referred to as the feminist issues. One other cluster of issues offers a base for comparison—the desirability of more or less spending on 11 national problems which have traditionally defined the liberal-conservative dimension. These are familiar concerns of health, education, welfare, crime, military, and the like.[3] Specific question wording for all of these items is in the Appendix.

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[1] As Snow, et al. (1980) have recently pointed out, there is good reason to believe that attitude consistency follows rather than precedes mobilization. Snow does not explain the belief consistency of core organizers, however, who presumably are in a state of permanent mobilization.

[2] For a general treatment of these issues and their place in the program of the movement for women's liberation see Freeman (1975).

The second step follows more closely the tradition of belief systems research. Assuming that political mobilization is accompanied by cognitive restructuring and greater consistency or constraint of attitudes, trends in the four attitude areas mentioned above are examined for increasing consistency over the decade. In the case of the feminist issue areas, consistency is also evaluated for the three issues combined. Consistency is evaluated in terms of the factor structure of the items and the standard deviations of aggregated indices.

The third step is to examine changes in the influence of the three feminist issues on self-identification as a liberal-conservative, party identification, and the presidential vote. The feminist issues are compared with the 11 items on federal spending. All of the policy issues are compared with party identification and ideological self-identification in predicting presidential voting in 1972 and 1976.

In the final analysis, I look for subgroup differences in the previously described trends. Subgroups are identified in terms of the structural characteristics which identify proponents and opponents of the new feminism. This analysis has been restricted to one type of subgroup—those who are frequent participants in religious services. Variables describing an individual's religiosity have proved to be the strongest predictors of differences between pro and antifeminist activists on the Equal Rights Amendment controversy (Mueller and Dimieri, 1982; Tedin, 1978).

DATA

The data examined are the 1972–80 General Social Surveys (GSS) from the National Opinion Research Corporation at the University of Chicago (NORC, 1980). These data are ideal in most respects in the search for trend items. The surveys are designed with the major goal of functioning as a social indicator program, and the exact wording is retained throughout the period in question. Items on the surveys are of two major types: permanent questions that occur on each survey, and rotating questions that appear on two out of every three surveys. The question rotation somewhat constrains the analysis in ways that will be indicated. Nevertheless, the GSS offers the best data source at present on attitudes related to the social and moral issues of interest.[4] Several other characteristics of the surveys which bear on this analysis should be mentioned, however.

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[3] These 11 items were chosen for analysis by Davis (1980) for the 1972–78 period because they were among the most volatile items in the General Social Survey (GSS). The more recent GSS from 1980 makes it possible to extend the analysis of these items by two years.

First, due to funding constraints, there were no surveys in the years 1979 and 1981. The absence of the 1981 survey, which would have included voting decisions in the 1980 election, is the most serious loss for present purposes. The timing of the surveys is also of relevance to

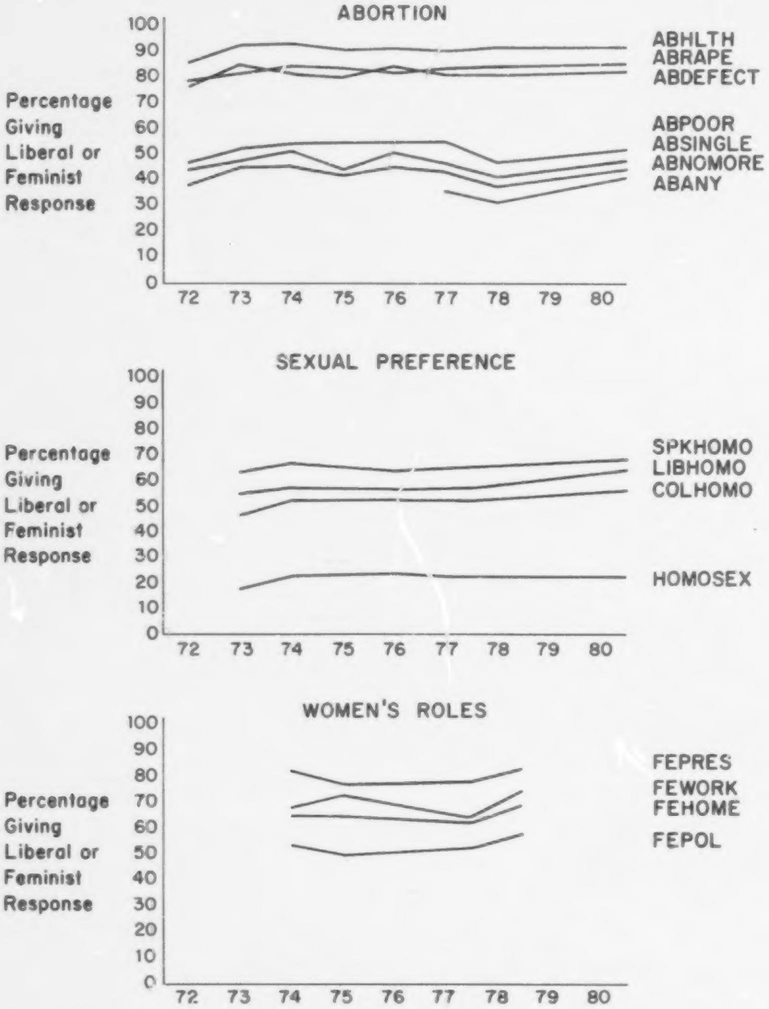

Figure 1. Trends in Attitudes Toward Three Feminist Issues, 1972–80

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[4] There are a total of 12,120 completed interviews. Each survey is an independently drawn sample of English-speaking persons 18 years of age or over, living in noninstitutional arrangements within the continental United States. Block quota sampling was used in 1972, 1973, 1974 surveys, and for half of the 1975, 1976 surveys. Full probability sampling was employed for the other half of the 1975, 1976 surveys, and for 1977, 1978, and 1980 (NORC, 1980:1).

the voting decisions of ultimate concern here. The GSS is conducted during February, March, and April, before most primaries are held. Thus, the responses reported here show the influence of attitudinal changes due to election year campaigning only in retrospect the following year. This is particularly important in the 1980 data, since the after-effects of the presidential campaigns in 1972 and 1976 are evident in subsequent surveys following those campaigns.

GENERAL TRENDS

Figure 1 indicates clearly that the discrete items representing the three feminist issues show no evidence of a conservative trend over the 1970s. In fact, for every item, the percentage giving a profeminist response is slightly higher in the spring of 1980 than it was in 1972. Despite slight variations of several percentage points, however, the dominant image is one of no change. The near-consensus which existed at the beginning of the decade on the desirability of abortions in cases of "extreme need"—the health of the mother is endangered (ABHLTH), the mother is the victim of rape (ABRAPE), or there is a strong chance of a serious defect in the baby (ABDEFECT)—persists with an average 4 percent gain into 1980. There is less consensus on "discretionary" abortions for reasons of low income (ABPOOR), the mother doesn't want to marry the father (ABSINGLE), the mother wants no more children (ABNOMORE), or any reason (ABANY). Yet, even here there is a slight feminist trend on every item.

Similarly, the civil rights of homosexuals to speak in public (SPKHOMO), to teach in colleges and universities (COLHOMO), and to have books advocating homosexuality appear in local public libraries (LIBHOMO) enjoy considerable public support. While only a minority of the public believe it is almost or never wrong for adults of the same sex to have sexual relations (HOMOSEX), this proportion has remained about 20 percent from 1973 to 1980.

The same absence of a conservative trend is found in attitudes regarding women's roles outside the home. Consistently strong support is shown for electing a qualified woman of one's own party for president (FEPRES). Although this endorsement is somewhat ambiguous, support is almost as strong for women's rights to participate in politics (FEHOME) and the paid labor force (FEWORK) and for their inherent capacity to engage in political activity (FEPOL). Like the other two issue areas, the change over the 1970s is slight (1 to 4 percent).

The absence of a conservative trend on feminist issues is not repeated, however, in attitudes toward spending on the national problems that more typically define the liberal-conservative dimension. Examination of these items (data not shown) reveals that although roughly two-thirds of the public continues to support spending for liberal issues such as health and education at approximately the same levels as the early 1970s, an even higher proportion supports more spending for conservative issues of crime and drug control. Support for redistributive issues of spending on the problems of the cities, race, and welfare declined sharply. The most precipitous change in all issues, however, is military spending. The public view that "too little" is spent on the military, armaments, and defense escalated from a small minority of 11.9 percent in 1972 to a majority position of 60.2 percent by 1980. Of the five issues that showed growing public support for increased spending over the decade, three—arms, space, and foreign aid—reflected a growing concern with the national image.

A comparison of the general trends over the 1970s between the feminist issues and the more traditional concerns that divide liberals and conservatives suggests that change in public sentiments has been stronger in the latter than the former.[5] Indeed, there has been a slight tendency for the public to move in a feminist direction on the issues of abortion, sexual preference, and women's roles outside the home. Thus, the general trends reveal little support for the growth of a conscience constituency focused on issues of private morality and the role of women. These general trends tell us little, however, about cognitive structuring—whether attitudes coalesced over the 1970s either within single issue areas or between issue areas.

Structuring of Beliefs

Converse's (1964) original operationalization of belief constraint was the average gamma over a set of policy issues taken two at a time. Increasingly, however, other researchers have utilized parametric measures of association and data reduction techniques such as factor analysis. Constraint is then indicated by high average correlations or simple factor structure where a single major factor accounts for a large proportion of the variance. As Stimson has indicated (1975), the objections to factor analysis as a means of identifying the degree of belief structure are more than offset by its advantages. This is particularly true when the same question items have been asked of the groups being compared.

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[5] Even these trends should not be overestimated. Davis (1980) has indicated that the essentially liberal rank ordering for these 11 spending items remained virtually unchanged (+.90) for the period 1973–78. Taylor et al. (1978) also report that support for black civil liberties showed no conservative trend during the mid-1970s.

FACTOR STRUCTURE

One way of examining the formation of a profamily or antifeminist consciousness is to determine whether the three issues—abortion, sexual preference, and women's roles—came to be seen as part of a coherent ideological position in the same way they are perceived, for instance, by pro-ERA activists (Mueller and Dimieri, 1982). The two GSS surveys which included all of the items for the three feminist issues were those for 1974 and 1977.

As indicated in Table 1, the data show no evidence that such coalescing of issues occurred over the decade. Four separate factors emerge to represent the three separate issues, with abortion subdividing into "discretionary" and "crisis" types. In both years, the

Table 1. Factor Loadings on Three Feminist Issues Combined, 1974, 1977

	Factors 1974				*Factors 1977*			
	1	*2*	*3*	*4*	*1*	*2*	*3*	*4*
"Discretionary" abortion								
ABNOMORE	.188	.784	.212	.098	.786	.195	.191	.128
ABPOOR	.100	.803	.259	.085	.787	.126	.275	.056
ABSINGLE	.153	.812	.223	.084	.786	.118	.242	.101
Sexual preference								
SPKHOMO	.835	.055	.066	.187	.050	.785	.075	.207
RLIBHOMO	.654	.091	−.011	.213	.122	.665	−.004	.216
RCOLHOMO	.787	.085	.021	.179	.081	.796	.045	.199
"Crisis abortions"								
ABDEFECT	.037	.202	.746	.059	.212	.050	.757	.106
ABHLTH	.044	.125	.712	.046	.123	.031	.723	.110
ABRAPE	.052	.279	.608	.094	.303	.051	.673	.063
Female roles								
FEHOME	.275	.081	.088	.708	.113	.242	.080	.782
FEPRES	.166	.083	.086	.500	.026	.136	.093	.500
FEPOL	.156	.021	.041	.687	.051	.169	.032	.628
Items below criterion								
RHOMOSEX	.448	.163	.050	.135	.237	.429	.035	.168
FEWORK	.275	.107	.131	.230	.130	.231	.102	.301
% Variance explained	30.5	16.9	9.5	8.2	31.6	16.5	9.8	7.9
Eigenvalue	4.27	2.36	1.33	1.15	4.42	2.31	1.37	1.11
Total variance explained	65.1				65.8			

four factors produce almost identical proportions of variance explained, eigenvalues for each factor, and loadings for individual items on each factor. There is no suggestion of items merging into a single factor by 1977 or of a reduction in the number of factors. The only change is a slight one—the switch in the first two factors between 1974 and 1977. Although the 1977 analysis gives greater prominence to the discretionary abortion reasons, this change does nothing to bring about greater coalescence among the three issues.[6]

These factor analyses suggest that a fairly high level of consistency existed within each of the separate issue areas at the beginning of the decade and that this consistency continued but did not generalize from one issue to another. Coherence continued to characterize attitudes within the three issues but not between them. The "single" issues remained "single" from the beginning to the end of the decade.

Correlational measures such as factor analysis do not tell us, however, whether the single issue consistency exists within individuals or within the public as a whole. It does not tell us whether a majority of people hold consistently moderate, consistently conservative, or consistently liberal positions (see Mueller and Judd, 1981). Nor does it tell us whether there might have been a shift in the positions of individuals over the decade so that a general liberalizing trend is offset by the hardening of attitudes at the conservative extremes. Individuals may have become more polarized in their thinking over the decade and, thus, more susceptible to political mobilization.

FREQUENCIES ON FEMINIST ISSUE SCORES

Individual scores were created for each of the three feminist issues from the factor solutions previously described. By coincidence, three items loaded above a .50 criterion for each issue. The scores are based on unit weighting whereby a 1 is assigned for a conservative answer on the following three items for the three issues:[7]

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[6] Since this analysis covers only a four-year period in the middle of the decade, the time frame is expanded by dropping the female role items and combining the abortion and homosexuality issues for 1973, 1976, and 1980. Yet, even for the full 1972–80 period there is no evidence of increasing simplicity of structure. The same three factors emerge as before for each year—discretionary abortion, sexual preference, and crisis abortion (data now shown). Total variance explained by the three factors remains high—70.6 percent, 72.2 percent, and 72.6 percent in 1973, 1976, and 1980—and the pattern of item loadings remains the same.

[7] Several substantive consequences follow from the decision to base the single issue scores on the factor solutions. For the abortion issue, it is the "discretionary" reasons for approving an abortion which loaded high on the initial factor accounting for most of the variance. Use of these items eliminates the three items which shared wide approval in our previous analysis. Thus, the score exaggerates the degree of disagreement on abortion. For the sexual preference items, we again eliminate the morality judgment on which there was nearly a conservative consensus and retain those items on civil

Abortion	ABNOMORE, ABPOOR, ABSINGLE
Sexual preference	SPKHOMO, LIBHOMO, COLHOMO
Female role	FEHOME, FEPRES, FEPOL

Thus, a 0 indicates that an individual had a perfect feminist or liberal score and a 3 indicates a perfect conservative score on each of the three issues. If individuals were moving toward more consistent conservative positions on the three feminist issues, the proportion of 3 scores should increase over the decade.

Once again, however, the data fail to support such expectations (Table 2). Although there is an increase in the liberal polar positions by 1980 of 4 to 9 percent, there is a *loss* in the proportion of people with a polar conservative position. This pattern is found for all three issues. For each issue the proportion of people with a perfect liberal score increases and a perfect conservative score declines. The standard deviations either decline or remain constant. Also, polarized distributions exist from the first time points for two of the three items. Thus, there is no support for the hypothesis that a conservative ideological position developed among individuals around antifeminist issues over the course of the decade. Instead, highly consistent posi-

Table 2. Individual Scores on Three Feminist Issues, 1972–80

		1972	*1973*	*1974*	*1975*	*1976*	*1977*	*1978*	*1980*
Abortion									
Scores	0	34.2	41.6	40.9	40.2	41.7	39.0	33.3	40.7
	1	9.8	8.2	10.1	9.2	8.1	10.8	8.6	8.4
	2	1.4	9.7	10.6	9.8	8.8	11.4	11.4	9.6
	3	45.6	40.5	38.4	40.9	41.4	38.9	38.9	41.3
SD		1.348	1.376	1.355	1.368	1.383	1.345	1.339	1.375
Sexual preference									
Scores	0		40.0	43.2		45.0	36.9		48.6
	1		17.6	17.7		14.8	15.4		14.5
	2		13.7	11.0		11.1	15.4		10.1
	3		28.7	28.0		29.0	32.4		26.7
SD			1.261	1.268		1.290	1.269		1.278
Female role									
Scores	0			45.2	45.3		43.2	49.3	
	1			21.2	21.2		21.4	21.5	
	2			18.2	19.1		20.6	17.9	
	3			15.3	14.5		14.8	11.2	
SD				1.117	1.106		1.107	1.056	

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rights—thus straying somewhat from our concern with moral issues. For the female role score, it is the item tapping the conditions of approval for women's labor force participation that failed to meet criterion. The three remaining items all measure some aspect of women's political participation. Thus, for each issue, the substantive meaning of the index is somewhat simpler than in previous analyses.

tions existed from the beginning of the decade on the issues of abortion and sexual preference. The strong support for changes in women's political participation skews that distribution in a feminist direction. Those in consistent opposition to women's right and capacity for a greater political role are a small minority from 1974 throughout the decade.

THE MEANING OF POLITICAL SELF-IDENTIFICATION

There is yet another way in which attitudes could have become restructured to indicate increasing politicization of beliefs on the three feminist issues here. Despite the fact that all tests indicate that the structure of these beliefs has remained the same, it is possible that ties to more traditional political issues and means of political self-identification have become stronger over the decade. For instance, the issues may have assumed increasing salience in the thinking of individuals as they consider their identification as a liberal or conservative, Republican or Democrat. To assess this hypothesis, two items from the GSS are used as dependent variables in multiple regression analyses where the three feminist issues are represented by the scores described previously. These are seven-item scales ranging from extremely liberal to extremely conservative and from strong Democrat to strong Republican. High scores represent extreme conservatism or strong Republican. The feminist issue scores are compared with the 11 items on national spending which are considered individually.[8] These comparisons are made for the selected years when items were available.

As indicated in Table 3, the three feminist items do have a signficant relation to individuals' conceptions of themselves as liberals or conservatives and as Democrats or Republicans. The magnitude of the R^2's in rows A and B indicate that the three issues have a considerably greater association with self-characterization as a liberal-conservative than with party identification. R^2's are two to eight times as large for ideological self-identification. Yet, in contrast with the national spending issues (row C), neither is very large. With the exception of 1974, the traditional spending issues explain several times as much of the variance in self-identification as the three feminist issues combined. Similarly, with party identification, the national spending issues account for 4 to 10 percent of the variance compared with only 2 percent or less for the feminist issues. Most significant for the question of whether the feminist issues have taken on increasing political significance over the 1970s are the differences between 1974 and 1977. As indicated in row B, there is a decline in

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[8] The absence of any strong or consistent factor structure among these issues suggests no rationale for combining them into a single score. Thus, each item is treated separately in this and subsequent analyses.

Table 3. Squared Multiple Regression Coefficients (R^2) for Self-Identification by Liberal-Conservatism and Party, 1973–80

	Liberal-Conservative Identification				*Party Identification*				
Items in Equations	*1974*	*1976*	*1977*	*1980*	*1973*	*1974*	*1976*	*1977*	*1980*
A. Abortion & sexual preference scores	.031*	.042*	.031*	.034*	.003	.009*	.005*	.006*	.0004
B. Abortion, sexual preference, and female role scores	.065*		.039*			.019*		.007*	
C. 11 items on national spending	.089*	.125*	.111*	.094*		.043	.044*	.059*	.094*
D. Combined items from A and C	.100*	.148*	.123*	.113*	.129*	.049*	.048*	.067*	.095*
E. Combined items from B and C	.129*		.126*			.057*		.068*	

* *p* .05.

the proportion of variance explained by the three feminist issues in terms of both political ideology and party identification. The three issues account for less rather than more of individual's self-identification over the decade.

This decline in the importance of the three feminist issues on two types of self-identification which characteristically account for a large part of the explanation of presidential voting decisions (Miller and Levitin, 1976) suggests that they would also have less rather than more influence on presidential votes from 1972 to 1976. Holm and Robinson (1980) have argued, however, that single issue groups are less likely to use traditional means of identification such as the liberal-conservative dimension in making their voting decisions. Thus, they argue that single issues should have a direct effect on presidential voting decisions unmitigated by the nature of their influence on ideological self-identification. There is reason, then, to evaluate the direct impact of the three feminist issues on presidential voting.

PRESIDENTIAL VOTING, 1972 AND 1974

As indicated by Table 4, the three feminist issues taken together are significantly related to the presidential vote in 1972 but not in 1976.[9] Once again, national spending priorities have an influence eight to ten

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[9] Once again, multiple regression is employed. While there is still some debate about the appropriateness of using multiple regression with a dichotomous dependent variable such as presidential voting, it is increasingly recognized that the overall R^2 is a reliable estimate of the total variance explained, although levels of significance attached to individual regression coefficients are unstable (Kenny, 1979; Kmenta, 1971). Thus, it is employed for present purposes where individual coefficients are not evaluated.

Table 4. **Presidential Voting for 1972 and 1976.[a] Squared Multiple Regression Coefficients for Issues, Party, and Liberal-Conservative Identification**

	McGovern vs. Nixon	*Carter vs. Ford*
A. Three feminist issues	.021*	.007^{NS}
B. 11 national spending issues	.161*	.075*
C. Party affiliation and liberal-conservative identification	.391*	.444*
D. C + A	.400*	.447*
E. C + B	.462*	.461*
F. A + B + C	.468*	.465*

* $p < .05$.

[a] The data for the 1972 election are from the 1974 survey and for the 1976 election from the 1977 survey. The survey years were selected with closest proximity to the election year and with all of the relevant items.

times as great as the feminist issues. As might be expected from a long tradition of research in political science, the best predictors of presidential vote are party affiliation and self-identification as a liberal or conservative. These two alone account for about half of the variance in the presidential voting decision. The contribution of neither the single feminist issues nor the national spending issues add significantly. Once again, we also see a decline in the political significance of the feminist issues over the decade.

The findings reported so far run contrary to the general understanding that a morally conservative trend has swept the country. Before this hypothesis is rejected, however, another possibility should be considered. It is still possible that the expected cognitive restructuring has occurred within important subgroups of the population which are too small to affect the national trends. There are many possible groups to consider—the politically conservative housewives who have opposed the ERA (Tedin et al., 1977), the morally concerned members of the "Old Right" (Wolfinger et al., 1964), the pockets of alienated individuals who oppose the secular trends in society (Page and McClelland, 1978), and the evangelicals who have received such a large share of media attention (Crawford, 1980). There is one tie, however, that links together all of the activists groups that have been identified by their opposition to issues of the women's movement. This is their deep involvement in religious institutions—both Protestant (Arrington and Kyle, 1978; Tedin et al., 1977) and Catholic (Mueller and Dimieri, 1982). Thus, rather than examine all of the possible subgroups, I shall focus exclusively on the intensely religious.

Beliefs of the Religiously Involved

Religious involvement is operationalized as those who attend religious services once a week or more. This group represents approximately one-third of the sample (from a high of 659 in 1972 to 516 in 1980). Rather than retrace the entire analysis with this group, I will examine three crucial aspects: the distribution of scores on the feminist issues over the decade, the strength of ties to the two types of political identification (liberal-conservativism and party affiliation), and presidential voting.

Table 5 indicates that, as anticipated, those who frequently attend religious services are considerably less feminist on the three issues than the general population. It also shows that most religiously involved individuals hold one of two polar positions on abortion and the rights of homosexuals—not just the conservative position. If Table 5 is compared with Table 2, we find that on the first time point for each issue, the proportion with a "perfect" feminist score is 8 to 14 percent less for those who attend religious service frequently. Yet, frequent attenders have not become more conservative. Instead, their rate of liberalization seems to lag behind that of the general public. Despite fluctuations in a conservative direction, the proportion with a "perfect" conservative score is no greater by 1980 than it was in 1972.

In contrast with the general public, however, self-characterization as a liberal-conservative as well as party identification are both predicted better by the feminist issues than by the national spending issues in 1974. Yet, by 1977, the pattern has reversed and, like the general public, national spending issues are once again more influential.

Those who are deeply involved religiously should also show stronger political significance for their moral beliefs if the claims for a

Table 5. Frequencies on Feminist Issue Scores for Religiously Involved, 1972–81

		1972	*1973*	*1974*	*1975*	*1976*	*1977*	*1978*	*1980*
Abortion									
Score	0	21.5	24.8	23.7	23.5	21.0	19.9	14.4	22.4
	1	9.0	6.5	8.9	8.8	5.8	9.6	5.7	7.6
	2	9.7	7.7	9.6	9.8	8.7	11.3	11.5	9.1
	3	59.8	61.0	57.8	57.9	64.4	59.2	68.4	60.9
SD		1.24	1.29	1.27	1.27	1.23	1.22	1.10	1.26
Sexual preference									
Score	0		30.8	34.3		34.0	30.8		36.5
	1		17.0	18.3		13.7	18.5		14.0
	2		15.8	12.7		13.5	16.2		12.1
	3		36.4	34.7		38.7	34.5		37.5
SD			1.26	1.28		1.31	1.25		1.32
Female role									
Score	0			37.2	38.5		36.6	38.7	
	1			24.4	21.2		21.9	22.7	
	2			20.7	21.6		21.7	21.9	
	3			17.8	18.7		19.8	16.7	
SD				1.12	1.14		1.15	1.12	

new "Religious Right" are to be accepted. The squared multiple regression coefficients (R^2) in Table 6 test this hypothesis. They show the relative effect of the three feminist issues on liberal-conservative and party identification as well as presidential vote for the elections of 1972 and 1976. The trend in the data from 1974 to 1977, however, parallels that for the general population presented in Table 3. The three feminist issues had a greater impact on political identity in 1974 than in 1977. In contrast with the general public, however, political self-characterization is better predicted by the feminist issues than by the national spending issues in 1974. Their joint contribution is almost completely additive (row C). By 1977, the pattern has reversed and, like the general public, national spending issues are once again more influential. In terms of presidential voting, the three feminist issues explain more variance for the religiously involved, but the difference from the general public is slight. The contribution of the national spending issues is twice as great as the feminist issues for the Nixon-McGovern election and four times as great for the contest between Carter and Ford. Following the pattern for the general public, the two forms of political identification explain an even greater proportion of the election variance for 1976 than for 1972. Thus, the increase in explained variance for the 1976 presidential election is due primarily to the increased salience of liberal-conservative identification. As indicated in the first section of Table 6, identification as a liberal or conserva-

Table 6. Squared Multiple Regression Coefficients (R^2) for Liberal-Conservative Identification, Party Identification, and Presidential Vote, 1974 and 1977[a] (Frequent Religious Service Attenders Only)

	1974	*1977*
Liberal-conservative identification		
A. Three feminist issues	.092*	.017$^{(NS)}$
B. 11 national spending issues	.061*	.096*
C. A + B	.146*	.104*
Party indentification		
A. Three feminist issues	.056*	.013$^{(NS)}$
B. 11 national spending issues	.048*	.080*
C. A + B	.100*	.100*
Presidential vote		
	McGovern-Nixon	Carter-Ford
A. Three feminist issues	.036*	.021*
B. 11 national spending issues	.087*	.088*
C. Liberal-conservative + party identification	.403*	.464*
D. A + B + C	.441	.492*

* $p < .05$.

[a] The survey years were selected which had the closest proximity to the presidential election years and contained all relevant items.

tive is increasingly accounted for in 1976 by national spending priorities—not moral issues.

Conclusions

Data have been presented to evaluate the hypothesis that an increasingly politicized "moral majority" emerged over the course of the 1970s. Using NORC General Social Surveys, this thesis has been evaluated using four different analytic strategies. All the findings run contrary to the hypothesis.

The basic findings are these: (1) there is no evidence for a conservative trend on three feminist issues for 1972–80; (2) factor analytic techniques reveal simple factor structure within the three issues but not between the three issues over the decade; (3) politicization of the issues is greater in 1974 than in 1977; and (4) the differences between the religiously involved and others have existed since the beginning of the decade and show no evidence of increasing political salience during the late 1970s.

From these findings, it seems clear that no pattern of increasing politicization of the three feminist issues had occurred by the spring of 1980. In fact, they seem more salient for the election of 1972, which has been frequently described as a heavily ideological contest because of the infusion of social issues into the campaign by McGovern's candidacy (see Nie et al., 1979; Miller and Levitin, 1976). It is likely, then, that the political salience of these issues is more dependent on the emphasis they receive from presidential candidates than on general cultural trends regarding approval or disapproval—even from social groups who might be expected to hold highly consistent positions. Thus, later data may indicate that the conflict over feminist issues at both party conventions in the summer of 1980 as well as the media attention devoted to the "New Religious Right" in the months before the election generated results more similar to the findings for the election of 1972 than that of 1976. Until such data are available, one must tentatively conclude that the "rise" of the Religious Right as a newly politicized moral consciousness has been overstated—as was its affect on the 1980 election (see Lipset and Raab, 1981).

Appendix

Question Wording on Attitude Items in Four Issue Areas

I. *Abortion:* Please tell me whether or not *you* think it should be possible for a pregnant woman to obtain a *legal* abortion if . . .

ABDEFECT If there is a strong chance of a serious defect in the baby?

ABNOMORE If she is married and does not want any more children?

ABHLTH If the woman's health is seriously endangered by the pregnancy?

ABPOOR If the family has a very low income and cannot afford any more children?

ABRAPE If she became pregnant as a result of rape?

ABSINGLE If she is not married and does not want to marry the man?

ABANY The woman wants it for any reason?

These questions were answered with a simple yes or no. In coding, yes received a 1 and no a 2. Don't knows were coded as missing values.

II. *Sexual preference:* "And what about a man who admits that he is a homosexual?"

SPKHOMO Suppose this admitted homosexual wanted to make a speech in your community. Should he be allowed to speak, or not? (Answers: 1=yes, allowed; 2=not allowed)

COLHOMO Should such a person be allowed to teach in a college or university, or not? (Answers: 1=yes, allowed; 2=not allowed)

LIBHOMO If some people in your community suggested that a book he wrote in favor of homosexuality should be taken out of your public library, would you favor removing this book, or not? (Answers are reversed so that: 1=not favor; 2=favor)

HOMOSEX What about sexual relations between two adults of the same sex—do you think it is always wrong, almost always wrong, wrong only sometimes, or not wrong at all? (Answers are reversed from 1–4. For most purposes they are collapsed into two categories where 1=wrong only sometimes or not wrong at all.)

III. *Women's Roles*

FEHOME Do you agree or disagree with this statement? Women should take care of running their homes and leave running the country up to men. (1=disagree)

FEWORK Do you approve or disapprove of a married woman earning money in business or industry if she has a husband capable of supporting her? (1=approve)

FEPRES If your party nominated a woman for president, would you vote for her if she were qualified for the job? (1=yes)

FEPOL Tell me if you agree or disagree with this statement: Most men are better suited emotionally for politics than are most women. (1=disagree)

Alternative wording for split sample in 1974: Would you say that most men are better suited for politics than are most women, that men and women are equally suited, or that women are better suited than men in this area? (1=men and women equal or women better)

All responses were dichotomous agree/disagree except for the alternative wording for FEPOL in 1974. These were reverse coded where necessary so that 1 was always the feminist response. "Don't know" responses were coded as missing values.

IV. *Spending for national problems:* We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount. First . . . we are spending too much, too little, or about the right amount on . . .

NATSPAC	Space exploration program
NATENVIR	Improving and protecting the environment
NATHEAL	Improving and protecting the nation's health
NATCITY	Solving the problems of the big cities
NATCRIME	Halting the rising crime rate
NATDRUG	Dealing with drug addiction
NATEDUC	Improving the nation's education system
NATRACE	Improving the conditions of blacks
NATARMS	The military, armaments and defense
NATAID	Foreign aid
NATFARE	Welfare

Responses were coded as follows: 1=too little; 2=about right; 3=too much. "Don't knows" were coded as missing values.

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Changing Patterns in American Voting Behavior, 1914–1980

PETER S. TUCKEL AND FELIPE TEJERA

ACCORDING to many political analysts, the effect of partisanship on vote choice has declined since the 1950s (Burnham, 1970, 1975; DeVries and Tarrance, 1972; Pomper, 1975; Nie et al., 1976). In this paper we attempt to show that this decline in partisanship is part of a more general process in which all ties which historically have connected one election to another have declined in importance. In addition to party loyalty, these ties include such factors as presidential coattails, the congressional midterm swing, and incumbency. The first factor (coattails) refers to the fact that candidates of the same party as the presidential candidate and running for different offices receive greater support in the presidential election year. The second factor (congressional midterm swing) refers to augmented electoral support for candidates of the out-party (i.e., the party which does not control the White House) during midterm elections. Finally, the third factor (incumbency) denotes that the same candidate for a particular office garners additional support in successive elections.

Abstract This paper analyzes voting behavior for President, Senate, and House at the state level for the period 1914–80. The major finding of this study is that since World War II, there has been a pronounced trend toward "particularization" in voting for federal offices. This trend consists of more than the erosion of party attachments among the electorate; it also includes the dissolution of other traditional electoral ties such as presidential coattails, the midterm congressional swing, and incumbency. As a result of this trend, the vote in federal elections is largely determined by factors which are unique to the specific setting in which each election occurs and by the ability of candidates to convert these factors to their political advantage through mass media campaigns.

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In this article, we use the term "particularization" to describe this more general process in which each election becomes an isolated event determined by the unique set of circumstances surrounding it. Specifically, this article aims to document the increasing trend toward particularization in voting in federal elections from 1914 to the present. The discussion is in five sections: (1) the data base, (2) the decline in communality in voting for federal offices during the same election year, (3) the decline in the effect of incumbency on the outcomes of U.S. Senate races, (4) an examination of the impact of campaign expenditures on senatorial voting, and (5) a brief overview of the findings.

The Data

The analysis is based largely on 83 series of data. Each series is made up of the proportion of the total vote received at the state level by a Democratic candidate or candidates: (1) Seventeen of the series are for the presidential vote, one series for each on-year election during the period 1914–80.[1] These series consist of either 48 or 50 observations, depending upon the number of states in the Union at the time. (2) Thirty-three of the series are for the vote in regularly scheduled Senate contests, one series for each even-numbered year during the period 1914–80. These series contain 30 to 34 observations, depending upon the number of states in the Union at the time and the number of states in which there was a regularly scheduled election during that year. (About two-thirds of the states hold Senate contests in each even-numbered year.) (3) The remaining 33 of the series are for the House vote, one series for each even-numbered year during the period 1914–80. Each series consists of either 48 or 50 observations, depending upon the number of states in the Union at the time. The value of each observation was obtained by dividing the total number of votes received by all Democratic candidates at the House level in a given state in a given year by the total number of votes cast for all House candidates in that state in that year.

The data for these 83 series were compiled from three sources. For the years 1914–70, the data were taken from Cox (1972). For the years 1972–78, the relevant editions of *Statistics of the Presidential and Congressional Election* (U.S. Congress, 1973, 1975, 1977, 1979) were consulted. The voting data for the most recent election were taken from the postelection issue of the *Congressional Quarterly*

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[1] 1914 was chosen as the starting point for this study because it was the first year in which all senators were popularly chosen.

Weekly Report (Nov. 8, 1980).[2] Sources of other data sets employed in this analysis are given in footnotes.

The Decline in Commonality in Voting for Federal Offices During the Same Election Year

ON-YEAR ELECTIONS

Let us begin by examining the common factors which connect the vote for federal offices (President, Senate, and House) during presidential election years. As a first step in this analysis, the pairwise correlations in the Democratic proportion of the vote for each office (President-Senate, President-House, Senate-House) were calculated at the state level for each presidential year.[3] After the correlations were calculated, a principal components factor analysis was conducted for each quadrennial election year from 1916 to 1980 and a single common factor was extracted for each year as well as the loadings for each of the three types of offices. This analysis was conducted for both the entire United States and the United States without the South.[4]

The analysis reveals two kinds of information: the strength of the common factor and the item loadings. The strength of the common factor, expressed as the proportion of variance explained, shows the degree to which voting for federal offices is tied together. In effect, the common factor measures the sum of two forces: party loyalty and coattails. The item loadings indicate the degree to which each electoral contest is affected by the common factor. In other words, the item loadings measure the influence which both party loyalty and coattails exert on the vote for separate offices.

Figures 1 and 2 plot the curves of the proportion of variance explained by the common factor over time for the entire country and the country with the South excluded. Both graphs show a marked

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[2] We would have preferred to use data at the individual as well as aggregate level in this study but no systematic body of survey data exists which extends over the entire period under investigation. The oldest collection of survey data of the type required for this analysis are the National Election Surveys conducted by the University of Michigan. These surveys begin in the post-World War II era and thus do not provide an adequate basis of comparison for the pre- and postwar periods.

[3] Each correlation was based on the same number of cases for a particular election year—the number of states which held races for all three offices in that year. This was done to insure comparability among the correlations.

[4] The South consisted of the 11 former Confederate States of America: South Carolina, Mississippi, Florida, Alabama, Georgia, Louisiana, Texas, Virginia, Arkansas, North Carolina, and Tennessee. The results of this analysis and the correlations upon which it is based are in Appendix I.

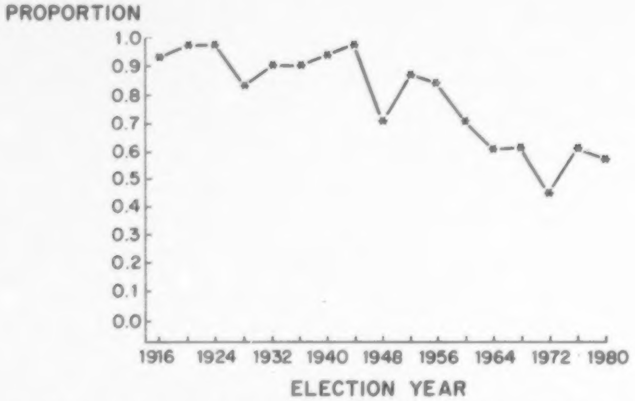

Figure 1. **Proportion of Variance Explained by a Common Factor in Presidential, Senate, and House Races for All States, 1916–1980**

decline in the strength of the common factor since the end of World War II. For the country as a whole, the common factor explained over 90 percent of the variance for seven of the eight pre-World War II elections. Since 1952, the proportion of variance explained dropped steadily until 1976, at which time it turned upwards. This swing upwards merely returned the level of the common factor to the 1968 level. The decline in the common factor continued again in the most recent election. The pattern for the country with the South omitted is similar.

The findings reported here are consistent with any one of three hypotheses: (a) the effect of traditional party attachments on vote choice has declined, (b) the impact of presidential coattails on the outcomes of House and Senate races has been reduced, or (c) both

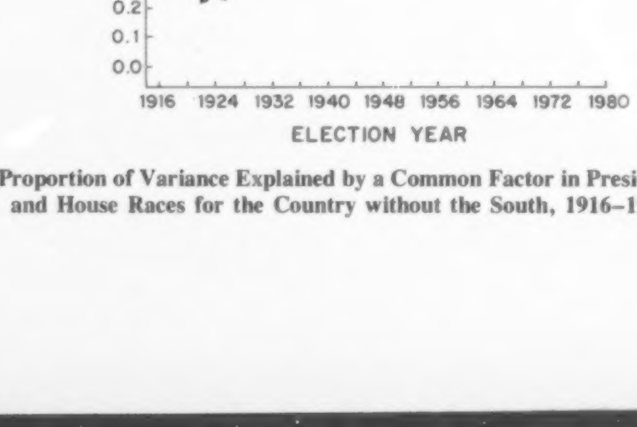

Figure 2. **Proportion of Variance Explained by a Common Factor in Presidential, Senate, and House Races for the Country without the South, 1916–1980**

the effects of partisanship and presidential coattails have diminished in importance. Given the magnitude and the consistency of the decline in the common factor, it is likely that either hypothesis (a) or hypothesis (c) is the most valid. If this is so, the above findings would tend to support the view that (1) since the conclusion of World War II there has been a trend toward declining partisanship among the voters and (2) the 1980 election represents a continuation of this trend.

The factor loadings listed in Appendix I are also noteworthy. In general, it is assumed that party loyalty and coattails (the two electoral forces measured by the common factor) exert the greatest influence at the House level. In fact, the proportion of the vote for House candidates received by the members of a given party is frequently used as a surrogate measure for the strength of that party in a particular state. The House loadings for the 1980 election is .37 for the country as a whole and .27 for the country with the South excluded. These are the lowest loadings for the House in the 17 presidential election years under study. Indeed, they are a quantum jump lower than the House loadings for the previsous 16 election years, which had never dropped below .70 before.

Both the low factor loadings for the House in the 1980 election and the low values of the common factor for that year (which were the second lowest for the 17 election years studied) are consistent with earlier findings based on survey data which suggest that the outcome of the 1980 elections was not the result of a major shift in party loyalties (Miller and Shanks, 1981; Pomper et al., 1981).

OFF-YEAR ELECTIONS

The next step in the analysis was to measure the strength of the common factor tying together the vote for Senate and House at the state level during midterm elections. The key factors linking the vote for these two offices would be party loyalty and midterm swing. An additional factor might be the influence of senatorial coattails (the effect of senatorial voting on House elections). Yet this influence is likely to be smaller than the influence of presidential coattails during on-year elections.

Since there are only two types of races held in the off-years (House and Senate), the proportion of variance explained by a common factor was computed by squaring the correlation between the Democratic proportion of the vote for these two offices. Figure 3 traces these squared correlations for the period 1914–78 for the country as a whole, and Figure 4 shows the correlations squared for the country without the South. (Appendix I lists the nonsquared correlations.) An

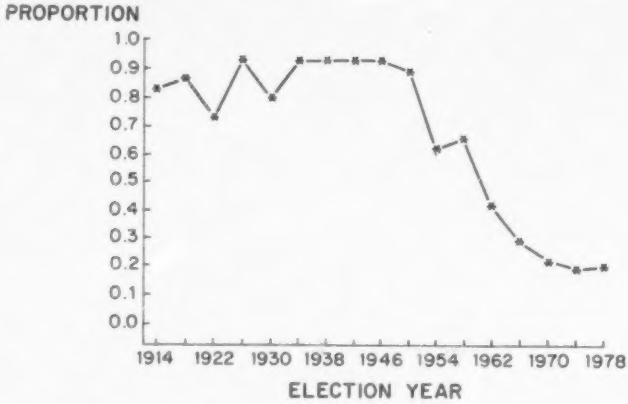

Figure 3. **Correlations Squared between Democratic Vote for Senate and House during the Off-Years for All States: 1914–1978**

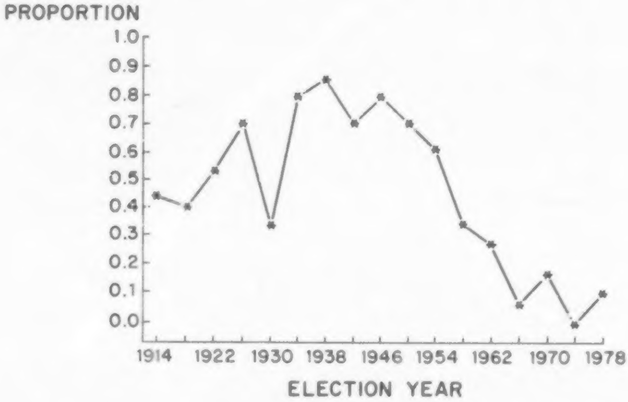

Figure 4. **Correlations Squared between Democratic Vote for Senate and House during the Off-Years for the Country without the South, 1914–1978**

examination of the two figures reveals the same general pattern which manifested itself during the presidential election years. Both graphs decline steadily in the post-World War II period. The correlations squared are lower in both graphs for every election in the 1958–78 period than for any election prior to 1958.

Taken together, the data in Figures 3 and 4 support the findings based on individual level research that the effect of partisanship on vote choice has declined.[5] In addition, the data provide evidence that the impact of coattails and the midterm swing on the outcomes of federal elections has been reduced.

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[5] The results presented in both Figures 3 and 4 were tested by means of the Mann Whitney U Test (1914–50 versus post-1950) and found to be significant at the .05 level.

PRESIDENTIAL COATTAILS AND THE CONGRESSIONAL MIDTERM SWING

The next step in the analysis was to measure in a more direct way the decline in influence of the two linking factors of coattails and the midterm swing. Previous research has shown that the winning presidential candidate often sweeps into office with him House and Senate candidates belonging to his party (Key, 1964; 543–53, 556–63). Our previous analysis suggests that the presidential coattails have been shortened in the modern period. To assess directly the effect of presidential coattails on the vote in federal elections over time, the gains and losses of House and Senate seats by the party which wins the presidency were tallied for each presidential year from 1916 to 1980[6] (Appendix II). These gains and losses were then averaged over two different time periods: 1916–48 and 1952–80. The results are displayed in Table 1. It is clear from the figures in the table that the effect of presidential coattails on the vote has been reduced with the passage of time, especially with respect to the House. During the first time period, the winning presidential party gained on the average 33 seats, but in the more recent period it gained an average of only 10 seats. The results for the Senate, though not as striking, reflect the same trend. In the early time period the overall change in membership of the winning presidential party was +4.6 and in the later period was +1.8. The evidence therefore supports the view that the effect of presidential voting on subpresidential voting, particularly at the House level, has declined.

The impact of presidential balloting on the vote for House and Senate is not restricted to on-year elections. It has long been noted by political analysts that the party out of power (the party which does not control the executive branch) generally increases its congressional membership during the off-year elections (Key, 1964:553–55, 563–64; Hinckley, 1967:694–700; Bibby et al., 1980:2). This midterm gain in

Table 1. Average Number of Senate and House Seats Won by Members of the Winning Presidential Party During On-Year Elections: 1916–1948, 1952–1980

Time Period	*Senate*	*House*
1916–48	+4.6	+33.6
1952–80	+1.8	+10.8

SOURCE: *Congressional Quarterly Weekly Report* (Nov. 9, 1974:3105; Nov. 8, 1980:3315, 3317) and *Statistical Abstract of the United States*, 100th Ed. (U.S. Department of Commerce, Bureau of the Census, 1979:507).

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[6] The gains and losses were calculated on the basis of Democratic party fortunes.

seats by members of the out-party, like the gain in seats during the on-years by members of the winning presidential party, has diminished in recent years. Table 2 shows the average number of seats gained by the out-party in two periods: 1914–50 and 1954–78.[7] (The year-by-year net gains are in Appendix II.) In the House, the average net gain in seats by members of the nonpresidential party is 40.9 for the period 1914–50 but drops to 27.4 for the period 1954--78. Comparable means for the Senate are 4.5 in the pre-1954 period and 2.7 in the post-1954 period. It is clear, then, that the advantage of the out-party during midterm elections has been narrowed in the modern period.

The Decline in the Effect of Incumbency in Senate Elections

Tables 1 and 2 show the decreasing impact of presidential coattails and the congressional midterm swing on the vote in federal elections. Both of these electoral forces have provided continuity in voting for federal offices during the same election year. A third linking factor—incumbency—has lent stability to the vote across different election years,[8] but the influence of incumbency in Senate elections also has waned in the last decade. Figure 5 presents the proportion of senatorial incumbents who achieved reelection in each biennial election from 1914 to 1980[9] (see Appendix II). Superimposed on these biennial

Table 2. Average Number of Senate and House Seats Won by Members of the Out-Party During Off-Year Elections: 1914–1950, 1954–1978

Time Period	*Senate*	*House*
1914–50	+4.5	+40.9
1954–78	+2.7	+27.4

SOURCE: *Congressional Quarterly Weekly Report* (Nov. 9, 1974:3105) and *Statistical Abstract of the United States*, 100th Ed. (U.S. Department of Commerce, Bureau of the Census, 1979:507).

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[7] Computations were based on Democratic party gains and losses.

[8] A number of earlier studies have demostrated that the incumbent candidate enjoys a sizable advantage over his opponent. For a discussion of the advantage of incumbency at the senatorial level, see, for example, Hinckley (1970) and Kostroski (1973); at the congressional level, see Erickson (1971) and Nelson (1978).

[9] The source of these data for the years 1914–70 was *The Biographical Directory of the American Congress 1774–1971* (U.S. Government Printing Office, 1971), and for the years 1972–80, the *Congressional Quarterly Weekly Report* (Nov. 11, 1972:2952, Nov. 9, 1974:3064; Nov. 6, 1976:3130, Nov. 11, 1978:3248; Nov. 8, 1980:3315).

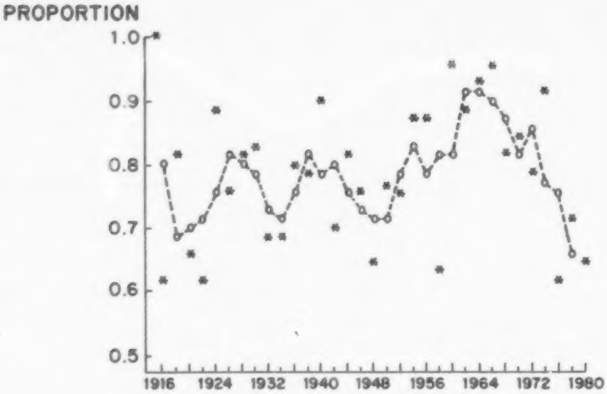

Figure 5. Proportion of Regularly Elected Senatorial Incumbents Who Win Reelection: 1914–1980

reelection rates is a three-point moving average, which indicates that the success rate of incumbent senators rises markedly in the two decades following World War II.[10]

This increase in the electoral safety of incumbents during the post-World War II period could be attributable to the change in the relative strength of the two major political parties which occurred between the 1950s and the 1970s and not so much to the fact that officeholding per se was becoming more of a political asset. During the decade immediately following World War II, the membership of the Senate was fairly evenly divided between the Democrats and the Republicans. Beginning with the election of 1958 and continuing for the next two decades, however, the Democratic party became the dominant party. Thus, the increase in the reelection rate of incumbent senators during this time period could be due largely to the improved electoral position of the Democrats.

Kostroski's analysis isolates the effect of incumbency from party strength on the vote in senatorial races during the years 1948–70 (Kostroski, 1973). The major conclusion of his study was that while party strength declined in influence, incumbency assumed greater importance. Hence, incumbency served as an independent source of electoral strength in this time period.

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[10] Figure 5 also shows that the success rate of senators plummets downwards in the most recent decade (including the year 1980), a point which we return to below. A similar pattern was observed by Bibby et al. (1980), who analyzed both the reelection rate of senators (based on the number who do not retire) and their margin of victory in the general election during the years 1944–78. The authors noted that up until the mid-1970s, both the proportion of senators who won reelection and those who won by a wide margin (at least 60 percent of the major party vote) tended to increase. In recent years, however, both of these indices of electoral security assumed lower values (Bibby et al., 1980:15, 16).

To examine the independent effects of party strength and incumbency on senatorial voting throughout the entire period under investigation, including the most recent decade, we conducted a statistical analysis similar to that of Kostroski's.[11] Three measures were obtained for each senatorial contest since 1914. The first measure was the percentage of the total vote for Senate received by the Democratic party candidate. The second measure was an indicator of the incumbent status of the two major party candidates in each race. This variable assumed a value of +1 if the Democratic party candidate was the current officeholder and −1 if the Republican party candidate was the current officeholder. A value of 0 was assigned to the variable if the seat was not being contested by an incumbent candidate. The third and final measure was an indicator of party strength at the state level. This variable was computed by averaging the Democratic statewide House vote in the two preceding biennial elections.

The Senate races were then grouped into six time periods. Each period, except the most recent, consisted of six election years coinciding with two complete senatorial cycles. A multiple regression analysis was then conducted for each time period with the Democratic percentage of the vote for senator as the dependent variable and party strength and incumbent status as the independent variables. The regression analysis was carried out for the total United States and for the United States with the South excluded. Table 3 presents the beta weights for this analysis which estimate the relative effects of the independent variables on the dependent variable. Inspection of these beta weights shows that the roles played by party and incumbency in influencing the vote for senator over time are similar in both analyses. Up to 1948, party was the predominant factor in influencing vote choice. Beginning in the postwar era and continuing through the early 1970s, the importance of party diminished and that of incumbency increased. In the period 1962–72, incumbency actually served as a more important determinant of the vote in Senate races than party.

These results are consistent with those reported by Kostroski. For the purposes of this research, the major finding is that in the most

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[11] Our analysis differs from Kostroski's in three respects. First, our indicator of party strength is geared toward measuring "average Democractic performance," whereas his indicator is geared toward measuring "core Democratic performance." Second, his analysis includes a variable to measure the effect of national partisan tides which we do not consider. (The effect of this variable, on the other hand, is incorporated in the error term of our statistical model.) And third, we conduct one analysis for both Democrats and Republicans and he undertakes two separate analyses.

Table 3. Results of a Multiple Regression Analysis Estimating the Effects of Party Strength and Incumbency on the Vote for Senator for the Total U.S. and for the U.S. with the South Excluded: 1914–1980

	Total U.S.			*U.S. without the South*		
	Beta Weights			*Beta Weights*		
Time Period	*Party Strength*	*Incumbency*	*Multiple R*	*Party Strength*	*Incumbency*	*Multiple R*
1914–24	.83	.11	.89	.43	.23	.55
1926–35	.82	.16	.91	.62	.25	.74
1938–48	.90	.03	.92	.71	.09	.77
1950–60	.76	.19	.88	.48	.32	.68
1962–72	.32	.53	.69	.14	.62	.67
1974–80	.39	.37	.58	.30	.41	.53

recent period (1974–80) the influence of incumbency has undergone a sharp decline. This finding accords with the results presented above in Figure 5 (see footnote 10) but demonstrates that the decline in the proportion of incumbents who achieve reelection cannot be attributed to the decreasing importance of party. Incumbents are more susceptible to defeat now not because the strength of the parties in their states has declined but because of their vulnerability as officeholders. The diminished role of incumbency in affecting the outcomes of Senate races provides yet another example of the dissolution of ties which bind together election in the contemporary era.[12] Table 3 also shows the multiple correlation between the independent variables and senatorial voting for each time period. These correlations measure the combined effect which party strength and incumbent status have on the vote choice for senator. Since 1948, there has been a monotonic trend downwards in the combined effect of the two independent variables. Together, party and incumbency have become considerably less important in determining the outcome in Senate elections.

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[12] While senatorial incumbents, as the above analysis has demonstrated, have experienced increasing difficulty in getting reelected over the last decade, House incumbents, by contrast, have continued to be reelected at phenomenally high rates. In the past, two explanations have been offered to account for the greater electoral viability of House incumbents. First, House members represent districts which are likely to be more homogeneous in their partisan composition than states. Second, voters might tend to evaluate House members more on the basis of their ombudsman activities and less on the basis of their policy stands than is the case with Senate incumbents. Recent research, however, has tended to emphasize a third factor underlying the greater electoral advantage enjoyed by House incumbents. This research points to the low levels of voter knowledge concerning House challengers as compared to Senate challengers (Abramowitz, 1978:633–40). In most House races the challenger is an obscure figure whereas in Senate contests the challenger enjoys considerable public exposure. The relevance of this finding to the present research becomes clear in the next section.

This finding underscores the general argument of this paper, namely, that historically existing continuities in electoral behavior have been disrupted in the modern era. The effects of party loyalty, coattails, the midterm congressional swing, and, in the case of senators, incumbency—all have diminished in importance.

If the traditional linking factors have assumed a less significant role in guiding electoral choice, then what new factor(s) is(are) operating? Two alternatives might answer this question. First, it is conceivable that some new linking factor (e.g., a new ideological force or issue not associated with either of the two major political parties) is now tying together the vote in different elections. Second, it could be that the particularistic factors surrounding each specific election are now the most important influences affecting the election outcome.

We cannot determine directly which of these two answers is the more valid, but we can test indirectly whether it is the particularistic factors associated with each election which are playing the dominant role in shaping electoral results. If it is the particularistic factors which are critical, then it follows that the ability of a candidate to exploit these particularistic elements should be of paramount importance in determining the vote. The ability of a candidate to exploit these particularistic factors, in turn, largely depends upon the funds he/she has available to spend in the campaign. Thus campaign expenditures should have a major influence on voting behavior if the particularistic hypothesis is valid. We turn now to an examination of the impact of campaign expenditures on voting in senatorial contests.

Campaign Expenditures and Election to the Senate

To assess the impact of campaign spending on the vote for senator, values on four variables were obtained for each regularly scheduled Senate election in the period 1972–78.[13] Three of the variables were the same as those employed in the preceding analysis (the Democratic percentage of the vote for senator, state party strength, and incumbency status). The fourth variable measured the Democratic proportion of total campaign expenditures.[14] A path model based on these four variables was then estimated and is presented in Figure 6. The path coefficients (standardized partial regression coefficients) over the arrows in the figure estimate the effect of a particular variable at the head of that arrow. From this model it is clear that the predominant influence on the Democratic proportion of the vote for senator is the Democratic proportion of campaign expenditures. Its path coefficient is over three times as large as the coefficients associated with the other two independent variables.[15]

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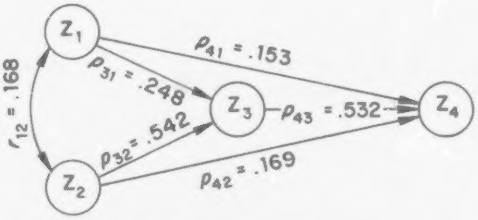
[13] 1972 was used as the beginning point for this particular analysis because it was the first year candidates for federal office were required to make detailed financial disclosures concerning the amount and sources of their campaign funds. The Federal Election Commission has not yet issued its final report on 1980 campaign spending and therefore this year is not included. Races in which there was no opposition were excluded.

[14] The values on this variable were obtained from data in the *Congressional Quarterly Weekly Report* (Sept. 22, 1973:2516; April 19, 1975:790–94; June 25, 1977:1293; Oct. 28, 1978:3115).

The model also demonstrates the advantage of the incumbent in raising campaign funds. The value of the path coefficient between incumbency and campaign expenditures is .542. When this path coefficient is multiplied by the path coefficient between campaign expenditures and the Senate vote, we arrive at an estimate of the effect of incumbency on the dependent variable mediated by the campaign expenditures variable. This indirect effect (.288) is greater than the direct effect which incumbency has on the dependent variable—.169. This finding indicates that the greatest advantage of the incumbent candidate over his opponent is his capacity as officeholder to obtain funds.



where z_1 = Democratic state party strength

z_2 = Incumbency

z_3 = Democratic proportion of campaign spending

z_4 = Democratic proportion of Senate vote

Figure 6. Path Model of the Vote For Senator

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[15] All path coefficients in the model were found to be significant, $p < .05$. This result is consistent with the findings reported by Jacobson (1978), who examined the effects of campaign spending in Senate elections during the years 1972–76. However, the focus of Jacobson's study differed somewhat from the focus of this analysis; therefore, Jacobson adopted a different methodology in his study. His objective was to provide a detailed evaluation of the impact of campaign expenditures on the vote for senator under differing electoral circumstances. Our objective is to determine the role played by campaign spending relative to the roles played by other electoral factors, such as incumbency status and state party strength.

Conclusion

This analysis has presented evidence of a general trend toward "particularization" in voting. In the past, voting behavior was subject to the constraints of party loyalty, presidential coattails, and the incumbency effect—all of which, in turn, lent a high degree of predictability to the vote outcome. These constraints no longer influence electoral behavior with the same degree of force as they did in the past. To an increasing degree the outcomes of federal elections seem to depend upon transient concerns and the ability of candidates to exploit these concerns through expensive, mass media campaigns.

This, then, raises the question: Why has there been a movement toward particularization in voting? While we have not addressed ourselves to this question in the present paper, we believe future research efforts aimed at answering this question should focus not only on the domain of electoral politics itself but on its relationship to the economic and more general social stratification system in society.

Appendix I

	With South							*Without South*						
Election Year	*Correlation*			*Factor*	*Item Loadings*			*Correlation*			*Factor*	*Item Loadings*		
	p-s	*p-h*	*s-h*		*p*	*s*	*h*	*p-s*	*p-h*	*s-h*		*p*	*s*	*ph*
1914			.92							.67				
1916	.89	.94	.93	.94	.97	.97	.98	.72	.65	.91	.85	.86	.96	.94
1918			.93							.63				
1920	.93	.96	.93	.96	.98	.97	.98	.78	.93	.67	.87	.98	.88	.94
1922			.86							.73				
1924	.90	.93	.95	.95	.97	.97	.98	.50	.77	.72	.79	.88	.83	.96
1926			.96							.84				
1928	.55	.80	.88	.83	.85	.89	.98	.24	.43	.82	.69	.62	.88	.95
1930			.90							.58				
1932	.80	.88	.95	.91	.93	.96	.99	.25	.45	.86	.71	.65	.88	.97
1934			.96							.90				
1936	.78	.86	.91	.90	.93	.95	.97	.09	.50	.74	.64	.57	.83	.95
1938			.96							.93				
1940	.85	.92	.96	.94	.95	.97	.99	.73	.71	.94	.87	.86	.96	.96
1942			.97							.83				
1944	.95	.94	.97	.96	.98	.99	.99	.81	.82	.91	.90	.93	.96	.96
1946			.96							.89				
1948	−.33	−.22	.94	.69	−.49	.97	.95	.76	.76	.81	.88	.94	.92	.96
1950			.95							.83				
1952	.70	.83	.93	.88	.90	.94	.98	.82	.81	.87	.90	.95	.94	.97
1954			.79							.78				
1956	.68	.72	.89	.84	.86	.94	.95	.69	.73	.76	.84	.92	.89	.94
1958			.82							.57				
1960	.31	.36	.89	.69	.57	.93	.95	.53	.73	.63	.76	.88	.82	.92
1962			.66							.50				
1964	−.20	−.18	.73	.60	−.42	.91	.90	.69	.60	.60	.76	.89	.88	.85

(Continued on page 244)

Appendix I. *Continued*

Election Year	*With South* Correlation p-s	p-h	s-h	Factor	Item Loadings p	s	h	*Without South* Correlation p-s	p-h	s-h	Factor	Item Loadings p	s	ph
1966			.55							.27				
1968	−.25	−.20	.72	.61	−.48	.90	.89	.39	.50	.53	.63	.74	.82	.82
1970			.48							.40				
1972	−.01	−.09	.35	.43	−.23	.80	.83	.21	.32	.19	.51	.78	.59	.77
1974			.45							.12				
1976	.32	.59	.34	.61	.84	.66	.85	.37	.49	.31	.59	.82	.71	.78
1978			.44							.29				
1980	.50	.36	.15	.56	.74	.57	.37	.53	.23	.17	.55	.71	.67	.27

NOTE:
p-s = correlation between Democratic vote for president and senator.
p-h = correlation between Democratic vote for president and congressman.
s-h = correlation between Democratic vote for senator and congressman.
p = item loading for office of president.
s = item loading for office of senator.
h = item loading for office of congressman.

Appendix II

Election Year	*Democratic Net Gains in Senate Seats*	*Democratic Net Gains in House Seats*	*Reelection Rate of Senators*
1914	+5	−59	1.00
1916	−3	−21	.61
1918	−6	−19	.81
1920	−10	−59	.66
1922	+6	+75	.62
1924	−3	−24	.88
1926	+7	+12	.75
1928	−8	−32	.82
1930	+8	+53	.83
1932	+12	+97	.68
1934	+10	+9	.68
1936	+6	+11	.80
1938	−6	−71	.78
1940	−3	+5	.90
1942	−9	−45	.70
1944	0	+21	.81
1946	−12	−55	.75
1948	+9	+75	.64
1950	−6	−29	.77
1952	−1	−21	.75
1954	+1	+19	.87
1956	+1	+2	.87
1958	+17	+49	.63
1960	−2	−20	.95
1962	+4	−4	.89
1964	+2	+38	.92
1966	−3	−47	.95
1968	−5	−4	.82

Appendix II. *Continued*

Election Year	*Democratic Net Gains in Senate Seats*	*Democratic Net Gains in House Seats*	*Reelection Rate of Senators*
1970	−4	+12	.84
1972	+2	−12	.79
1974	+3	+49	.91
1976	0	+1	.62
1978	−3	−12	.71
1980	−12	−33	.64

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Sampling by Ethnic Surnames: The Case of American Jews

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BECAUSE members of minority groups constituting a very small population of a national population are difficult and costly to locate using standard probability sampling, social scientists interested in studying small minority groups have frequently had to rely on nonprobability sampling methods. Some of the most common ways of obtaining rare population subjects have been by screening households in known minority communities, recruitment from lists of persons known to belong to the group, such as minority group organization lists, and selecting subjects on the basis of definitive demographic characteristics, such as physical appearance, language, and names. Yet the use of such techniques makes the representativeness of the selected sample and the research results questionable.

If research projects on small population groups must rely on nonprobability methods of sampling, therefore, an attempt should be made to determine which of those methods yields the most unbiased

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Abstract It is often difficult and costly to locate members of numerically small minorities using standard probability sampling. Consequently, nonprobability sampling techniques of various sorts are commonly used. This paper analyzes the differences between samples chosen by two such techniques—the use of ethnic surnames and the use of organization lists—and compares them with probability samples. Using data from the National Jewish Population Study, we find that Jews with one of 35 so-called distinctive Jewish names do not differ substantially from the general population of Jews in demographic characteristics or indicators of Jewish identification, and that this technique produces a sample which is more similar to the general population of Jews than does the organization list sample technique.

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estimates of actual population characteristics. This paper examines the differences between a probability sample of a small population group (American Jews) and two nonprobability methods of sampling them—the use of distinctively Jewish surnames and the more commonly used method of organization membership lists.

Samples of American Jews

American Jews are a good example of a small minority group that has received considerable research interest and yet study after study is plagued with the problems of unrepresentative samples. Jews constitute a small proportion of the American population and in recent decades that proportion, now estimated to be between 2.6 percent and 2.8 percent nationally (Massarik, 1974), has been declining. The U.S. Census does not ask a question on religion, and since surveys by national polling agencies yield very few Jews, some of the major sources of population data used by social scientists to study Americans have not been adquate for the study of American Jews. Consequently, most studies in this area have used nonprobability samples of an accidental or purposive nature. The most common sample is usually taken from some organization list or combination of lists which contain Jewish names and addresses. In small Jewish communities where affiliation rates for Jews are as high as 90 percent, the use of organization lists does not present big problems. However, in large Jewish communities, where most American Jews live, these lists necessarily bias the sample toward affiliated Jews, which we now know are only about half of all adult Jews in the United States (Bock, 1976; Lazerwitz, 1979). Thus, there is serious doubt about the generalizability of many of the findings.

In the last decade and a half, there have been numerous Jewish community censuses which use a combination of list and area housing samples.[1] There was also the National Jewish Population Study (NJPS), which is based on a complex multistage, national probability sample (Lazerwitz, 1973). These large-scale surveys have been expensive and typically beyond the financial means of individual researchers working in the field. Thus, there is still need for some relatively inexpensive, efficient method of screening the population in order to obtain a representative sample of Jews for various types of research concerns. One method which might be adequate for this purpose is a sample of individuals with distinctive Jewish names (DJNs).

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[1] See Appendix in Elazar (1976) for a listing of community studies before that date. Contact the Council of Jewish Federations, 575 Lexington Ave., New York, NY 10022, for a list of more recent studies.

The Development of DJN Research

The DJN method has been around for quite some time and has been used by demographers to identify geographic clusters of Jewish populations. While there have been several working papers and unpublished reports on the method, to our knowledge there has been only one published paper on the subject (Massarik, 1966) and it does not fully explain the development of the DJN list. Moreover, there is only one copy available of the original report (Kohs and Blumenthal, 1942) on the development of the method. Therefore, a brief history is in order.

In 1942, Samuel C. Kohs compiled a list of the most common names appearing in the Los Angeles Jewish Federation files. He found that 106 surnames accounted for about 15.54 percent to 17.76 percent of the names on various Jewish Federation lists. Overall, his list accounted for 16.24 percent of the total names on all lists. Upon further analysis, Kohs found that there were 35 names[2] which appeared 10 or more times on each of the lists. These 35 names accounted for 11.55 percent of the total names on all of the lists. Generally, about 90 to 92 percent of persons with these 35 names are Jewish, although this might drop as low as 70 percent in some locales. Thus, by using only 35 names, the percentage of all Jews included is not much less than by using all 106 names, but the probability of actually sampling a Jewish person is very high. In a sense, then, these particular names are not only "distinctive" they are also "common." There are names not included in the list which might yield even higher proportions of Jews but persons with those names are fewer in number.

Massarik (1966) reports that with some local variation the 35 DJNs constitute a relatively constant proportion of total Jewish persons, which he sets at 12 percent. Therefore, Massarik has argued, the DJN can be used to get a fairly accurate estimate of the total size of the Jewish population, achieving greater accuracy in areas with dense Jewish populations than in low-density areas. Lazerwitz (n.d.) reports that in the 1971 National Jewish Population Study (NJPS), 15.5 percent of the Jewish respondents had the 106 DJNs. In fact, the Jewish identification sample of the NJPS to be used in this paper had 14.5 percent of the sample with 106 DJNs and 11.4 percent with 35 DJNs. Thus, the proportion of Jews with DJNs has indeed remained relatively constant over time and nationwide. Of course, as everyone who has used the DJN method has noted, estimates of Jewish population size require some knowledge of the proportion of the DJN in the general population and also the proportion of DJN and general population who might not be included in whatever name list is being used (e.g., phone directory).

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[2] Berman, Bernstein, Caplan, Cohen, Cohn, Epstein, Feldman, Friedman, Ginsberg, Gold, Goldberg, Goldman, Goldstein, Greenberg, Grossman, Horowitz, Kahn, Kaplan, Katz, Levin, Levine, Levinson, Rosen, Rosenbaum, Rosenbloom, Rosenthal, Rothman, Rubin, Samuels, Shapiro, Siegel, Silverman, Weinberg, Weiner, Weinstein.

Over the years a number of studies of American Jews have chosen their samples by using a list of names considered by the researchers to be distinctively Jewish but they have not always stuck to the original DJN list exclusively (Rosenthal, 1948; Himmelfarb, 1974; Mayer, 1979; and Jaret, 1978). Most recently, DJN was used as a sampling strategy for the New York Area Jewish population. Ritterband and Cohen (1978) suggest that by identifying phone exchanges where Jewish households are clustered (with clusters identified by Jewish names), the number of exchanges needed for random-digit-dialing could be cut by three-quarters. This achieves great economies while excluding only about 10 percent of those Jews whose phone exchanges are not in the cluster of Jewish households. Thus, sampling by names has been fairly common and continues to be advocated as a cost-saving method. It has even been used for demographic studies of Jews in other countries (DellaPergola, 1977).

A More Direct Use of DJN Samples

Past research with DJN has concentrated on using the method to identify where Jews live and estimating Jewish population size. Rather than using DJNs to locate other Jews to be sampled, for some purposes it would be more economical, and perhaps as efficient, to actually sample only persons with DJNs. Such a sample might be less biased than the commonly used samples of Jewish organization lists.

To date, there has been no research on the extent to which Jews with DJNs are representative of Jews generally. Two limitations seem obvious. First, it is quite clear that Jewish women who are married to non-Jewish men are likely to be unrepresented in a DJN sample. With rising intermarriage, this might be of some concern. However, this problem is minimimzed somewhat by the fact that Jewish females are only one-third as likely as Jewish males to marry someone of a different faith (Lazerwitz, 1981). Second, we might assume that persons with DJNs are more likely to be foreign-born, older, and have higher levels of Jewish identification than Jews generally. If these or other differences exist between DJN Jews and Jews generally, it is important to know the magnitude of the differences, because the bias resulting might be less than is typical in other list samples of Jews. It is our intention in this study, therefore, to investigate just how representative DJN Jews are of the American Jewish population generally.

Method

SAMPLE

The data used for this study were taken from the National Jewish Population Study collected between the spring of 1970 and December 1971. The full explanation of the sampling method has been described by Lazerwitz (1973 and 1978). Briefly, households were selected in a two-stage process. Organizational lists were obtained and sampled for each primary sampling unit. Then area probability samples were selected in each PSU and integrated with list samples so that there was no overlap between the list and area samples. The households were screened for Jewish occupants, and ultimately 7,179 Jewish households were sampled nationwide. Of these households 5,790 were successfully interviewed for a 79 percent response rate. Despite the length of time that has elapsed since the survey was taken, the NJPS is still the most representative national data available on American Jews. Of the 5,790 housing units, our concern lies with 3,997 respondents who answered a special section of the survey dealing with Jewish identification. One adult Jewish resident per household (per two households in New York City) was selected via the Kish technique to complete the identification section. The weighted *N* for this section of the survey is 8,544, which is the *N* used here except for variations on particular variables due to missing responses.

MEASURES

To investigate the differences between DJN persons and other Jews, we selected eight measures of background characteristics, nine single-variable measures of Jewish identification, and eight Jewish identification scales. The specific single-variable measures were chosen because they are commonly used indicators of Jewish identification and because they range across the diversity of modes of identification in the religious, communal, social, and cultural spheres (see Himmelfarb, 1975): rituals, synagogue membership and attendance, Israel, charity, friendships.

The Jewish identification scales were also developed to encompass a diversity of types of identification and to give more reliable indicators of those types than can be obtained from single measures. The development of the scales was rather complicated. Briefly, 31 items measuring Jewish identification were factor-analyzed using a principal component solution. This yielded eight orthogonal factors. One of those factors measured contributions to Jewish charities, but had too many missing values to be used. Thus, we were left with seven factors from which scales were formed:

Devotional—ritual observance.
Associational—Jewish organizational participation.
Fraternal—living near, and associating with, other Jews.
Ideological—attitudes toward Israel.
Intellectual—knowledge of Jewish culture, history, religious teachings, and ethics.
Esthetic—enjoyment of Jewish music and literature and possession of Jewish art objects.
Affectional—attitudes about the Jewish people and the religion.

Finally, the seven scales were rotated and yielded only one factor. Therefore, an overall measure of Jewish Identification (JI) was formed by weighting the separate scales by their factor score coefficients and adding them together. (See the Appendix for a more complete explanation of the construction of these scales as well as the survey questions from which scales and the single-variable measures of Jewish identification were derived.)

The background variables were as follows:

Number of children in household—number of persons in household under 21 years of age.
Age—from 19 on up (only adults were used in the Jewish Identification subsample).
Sex—(1) male; (2) female.
Education—in completed years.
Family income—in 17 categories from low to high with numbers between 0 and 50.
Generation American—(1) foreign-born; (2) respondent American-born but had at least one foreign-born parent; (3) respondent American-born and both parents American-born; (4) respondent and parents American-born and at least one grandparent American-born
Jewish education—total hours of Jewish schooling, estimated by multiplying the estimated hours spent on Jewish studies in each type of Jewish school by the number of years in that school type. (For the hour estimates of each school type see Bock, 1976.)

Denomination—(1) Orthodox and Traditional; (2) Conservative and Reconstructionist; (3) Reform; (4) other, agnostic, atheist and "Just Jewish."

Findings

Previous analysis (not shown here) found that the 35 DJN were more different from the rest of the Jewish population than the entire list of 106 DJN. However, in general the differences between each of the DJN lists and the rest of the Jewish population were quite small. Therefore, for purposes of this paper, we will present only the comparison for the DJN group with largest differences, i.e., the 35 DJN and all other Jews in the sample.

Table 1 shows the mean differences in background characteristics between Jews with one of the 35 DJN and all other Jews in the sample and between the list sample and area sample Jews. It also shows the significance levels for: the t-tests of the differences between the means, and the Spearman rank-order correlation between background and name-type (DJN=1, others=0) and between background and sample-type (organization list=1, area sample=0).

Looking at the background variables, we see that DJN Jews are: a little younger, more likely to be female, less secularly educated, more wealthy, and, surprisingly, more native American than are other Jews. They also have a little more Jewish education and are less liberal denominationally. However, a look at the significance levels shows that only the differences in age, generation, education, and

Table 1. Mean Differences in Background Between Jews with DJNs and All Other Jews and Between List Sample and Area Sample Jews

	35 DJN[a]	*All Others*[b]	*Spearman r*	*List Sample*[c]	*Area Sample*[d]	*Spearman r*
N children in household	.93	.89	.02	1.07	.81*	.10^{+}
Age	43.3	48.6*	−.10^{+}	50.5	46.6*	.11^{+}
% female	56	54	.01	57	53*	.03
Secular educ. (yrs.)	14.1	13.8*	.01	14.0	13.7*	.01
Income	14.8	14.4	.00	18.4	12.3*	.22^{+}
Generation	2.5	2.4*	.03^{+}	2.4	2.4	−.01
Jewish educ. (hrs.)	11,433	10,037	−.02	13,310	8,401*	.09^{+}
Denomination (Orthodox to other)	2.7	2.9*	−.01	2.7	3.0*	−.08^{+}

* t-test for differences between means significant at least at .05 level (two-tailed).

$^{+}$ $p \leq .001$.

[a] N ranged from 875 to 948 except for income where N dropped to 637.

[b] N ranged from 7,373 to 7,596 except for income where N dropped to 5,665.

[c] N ranged from 2,896 to 2,987 except for income where N dropped to 2,191.

[d] N ranged from 5,395 to 5,549 except for income where N dropped to 4,106.

denomination are statistically significant. The Spearman correlation is significant only for age and generation. In any case, all of the correlations besides age are very low.

It is no surprise to find that the DJN sample fares better in approximating the composition of a more general sample than does the sample drawn from organization lists. A look at the last three columns of Table 1 shows that the list sample differs dramatically from the area sample drawn from organization lists. A look at the last three columns of Jewish education, and especially income. The Spearman correlations are significant for every variable except sex, secular education, and generation. The differences between the list and area samples are almost always substantially greater than the differences between the DJN sample and all others.

Table 2 provides similar comparisons between the Jews with DJNs and all others and between the list and area samples with regard to the various measures of Jewish identification. We expected to find that Jews with DJNs would be persons with stronger Jewish identification. However, our expectations were confirmed in only a few instances. The first three columns of Table 2 show that the DJN Jews are a little more ritually observant than the others, a slightly higher percentage gave to a Jewish charity, and more of them are members of a synagogue. On the identification scales, DJN Jews score higher than the others on the devotional and intellectual identification scales; however, they score lower than the other Jews on all of the other scales, including the overall measure of Jewish identification (JI). A few of the mean differences and Spearman correlations are statistically significant for the single-variable measures of Jewish identification, though in most cases the mean differences are quite small (and significance levels are easily achieved with such large samples). Though the differences are greater for the scale measures, the Spearman correlation is only of notable magnitude for the fraternal scale. Apparently, DJN Jews are less likely to have Jewish friends, neighbors, and business associates.

A look at the last three columns of Table 2 shows that the organization list sample less adequately approximates a more general population of Jews than does the DJN sample. On most measures of Jewish identification, the list sample differs from the area sample to a greater extent than the DJN sample differs from the "all others" sample. (The only notable exception is the fraternal scale.) Affiliated Jews (the list sample) are considerably more observant and have a stronger Jewish identification than the others, particularly on affiliational measures, i.e., synagogue membership and activeness in Jewish organizations (associational scale).

Table 2. Differences in Jewish Identification Between Jews with DJNs and all Other Jews and Between List Sample and Area Sample Jews

	35 DJN[a]	*All Others*[b]	*Spearman r*	*List Sample*[c]	*Area Sample*[d]	*Spearman r*
Selected measures[e]						
% for whom all or most friends are Jewish	63	68*	−.03	74	64*	.11[+]
% who have been to Israel	17	15	.01	20	13*	.10[+]
% who gave to a Jewish charity	98	95*	.05[+]	98	93*	.11[+]
% who attended Seder last year	87	82*	.04[+]	85	81*	.10[+]
% living in household that observes kosher	30	28	.01	32	27*	.06[+]
% who said Kiddush last Sabbath	20	19	.01	20	18*	.03
% who observed Sabbath in some way	50	37*	.08[+]	43	36*	.08[+]
% who attend services on all or most weekends	5	6	.00	8	5*	.06[+]
% who belong to synagogue	60	47*	.08[+]	67	39*	.27[+]
Jewish ID scales (means)						
Devotional	.15	−.02*	.06[+]	.06	−.03*	.04[+]
Associational	−.07	.01*	−.07	.27	−.14*	.21[+]
Fraternal	−.23	.03*	−.09[+]	.04	−.02*	.01
Ideological	−.07	.01*	−.02	.18	−.10*	.14[+]
Intellectual	.05	−.01	.04[+]	.04	−.02*	.03
Affectional	−.18	.00*	−.04[+]	.09	−.08*	.07[+]
Esthetic	−.09	.01*	−.04[+]	.09	−.05*	.07[+]
Total JI	−.08	.02*	−.04[+]	.19	−.09*	.12[+]

* *t*-test for differences between means significant at least at .05 level (two-tailed).

[+] $p \leq .001$.

[a] *N* on selected measures ranged from 735 to 876 except for charity question, where *N* dropped to 502. *N* on scales ranged from 909 to 948.

[b] *N* on selected measures ranged from 6,000 to 7,659 except for charity question, where *N* dropped to 5,212. *N* on scales ranged from 7,053 to 7,596.

[c] *N* on selected measures ranged from 2,409 to 2,983 except for charity question, where *N* dropped to 2,294. *N* on scales ranged from 2,794 to 2,987.

[d] *N* on selected measures ranged from 4,320 to 5,547 except for charity question, where *N* dropped to 3,414. *N* on scales ranged from 5,760 to 5,549.

[e] The percentages shown are actually the mean values of variables dichotomized into the classification of 0 to 1.

Even these differences, however, are not as great as one might expect, and this might encourage some researchers to continue using organization list samples. We would argue that the differences in Jewish identification coupled with the differences in demographic characteristics (particularly in income) are sufficient to caution researchers against the list method, especially in light of our findings that DJN Jews are a readily available sample and generally quite similar to representative samples of the American Jewish population.

Conclusions

Common sense would lead us to believe that Jews with distinctive Jewish names are likely to be older, to be foreign-born, and to have a stronger Jewish identification than other Jews. In fact, the opposite is true. Apparently, name changing was more common among the immigrant generation. American-born Jews, although not identifying in traditional Jewish ways, are comfortable enough in their American status to maintain names which are distinctively Jewish in character.

The most important point of our findings, however, is not the direction of the differences which exist between those with DJNs and other Jews, but the fact that the differences are very slight and considerably less than those which are obtained when samples are from Jewish organizational membership lists. Therefore, we believe that a random sample of persons with DJNs is likely to produce a fairly representative sample of American Jews. The low cost of sampling only DJN Jews, particularly for explanatory rather than descriptive studies (i.e., studies which are interested in explaining patterns rather than making accurate estimates of population characteristics), and particularly for studies not concerned with interfaith marriages or immigrant experiences, is likely to outweigh the slight unrepresentativeness which might result.

Several caveats might be noted to this conclusion:

1. We have used a national sample and have not looked at possible geographic differences in the characteristics of DJN persons. Given the minimal differences here, it is likely that differences will be minimal in any large-size Jewish community. However, in small Jewish communities, where persons with DJNs are perhaps more likely to be related to each other, and where there are many fewer persons from which to choose, DJN Jews might differ from other Jews to a much greater extent. This needs to be researched further.

2. Sampling Jews with DJNs implies using some existing list of names and addresses such as a phone or house directory. All such lists have limitations which might produce substantial biases (e.g., excluding large numbers of persons with new phone numbers, unlisted

phone numbers, or those without phones) regardless of who is sampled from them. These inherent biases should be considered when weighing the advantages and disadvantages of sampling from those lists.

3. Sampling from the DJN list presents additional biases: Jewish women married to men who were not born Jewish are not likely to have a Jewish surname and therefore will be underrepresented in DJN samples. Similarly, of course, males who have converted to Judaism will be underrepresented by this sampling method. The names on the DJN list are primarily characteristic of Eastern European Jews. Thus, Sephardic Jews (those from Mediterranean, Asian, and African countries), possibly new Russian immigrants, and many recent Israeli immigrants (the largest new Jewish immigrant group to America) are also likely to be underrepresented by this method. Where there are large settlements of these groups, DJN sampling should be used with caution, perhaps by modifying the name list to include names which are commonly found among these immigrants.[3]

The DJN list was developed almost four decades ago. Its full potential as a research tool has yet to be reached.[4] Despite the warnings above, we believe that the benefits of DJN sampling are likely to be substantial and that our research indicates that many of the feared limitations are likely to be slight.

The distinctive name method also needs to be developed for use in research on other numerically scarce ethnic groups. For example, there is impressionistic evidence that name lists of Greek-Americans, Chinese-Americans, and Korean-Americans would be even more efficient than the name lists developed for use in research on Jews. There is a great need for developing low-cost sampling procedures particularly for research on American religious denominations and ethnic groups. Further work on name sampling might rapidly advance that development.

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[3] In fact, Ritterband and Cohen (1982) report that in comparing their own version of a DJN list of Jews to the other Jews that were sampled in their New York Jewish population survey, all differences were negligible, except for intermarried Jewish women and Israeli and Russian-born Jews.

[4] One ambitious advertising service has collated Jewish names from subscription and organization lists. They now have some 27,000 names and addresses on their computer and can establish the likely proportion of persons with those names who are Jewish within particular locales. At an earlier stage of development, this firm provided researchers in New York with 600 names, which constituted 31 percent of Jewish names in the New York Jewish federation files. Researchers were then able to estimate the number of Jews in an area by weighting the DJNs by this factor (Ritterband and Cohen, 1982).

Appendix

SINGLE-VARIABLE MEASURES OF JEWISH IDENTIFICATION

The single variable measures of Jewish identity were derived from the following survey questions:

Percent for whom all or most friends are Jewish: Among my friends [none, few, some, most, all or almost all] are Jewish.

Percent who have been to Israel: Have you ever been to Israel?

Percent who gave to a Jewish charity: In 1969, did [head] make any contributions to any charitable or welfare campaigns? [If yes] Not counting dues, of the total amount contributed by [head] in 1969, about what percent was donated to Jewish charities?

Percent who attended Seder last year: In 1969, did you attend a Seder?

Percent living in household that observes kosher: Is "kosher" observed in this household?

Percent who said Kiddush last Sabbath: Was the Sabbath observed in this household last week? [If yes:] How was the Sabbath observed?

Percent who observed Sabbath in some way: Was the Sabbath observed in this household last week?

Percent who attend services on all or most weekends: How many times, if any, have you attended Jewish religious services during the past 12 months? [If any:] On what occasions was that?

Percent who belong to a synagogue: Are you a member of one or more temples or synagogues?

JEWISH IDENTIFICATION SCALES

Except for the Devotional and Ideological scales, all other scales of Jewish Identification (JI) used in this study were formed as composite factors. All the items used in a scale and their corresponding factor weights are listed below. The Devotional and Ideological scales were formed as Guttman scales by successively weighting each item with an additional point from the least difficult item to the most difficult item (see the procedure outlined in the *SPSS: Statistical Package for the Social Sciences* manual). The percentage of respondents "passing" an item is listed next to it.

Devotional (Guttman scale)

82%	Attended a Seder last year—1969
70%	Lit Chanukah candles
52%	Had only matzah in the home on Passover
36%	Lit Sabbath candles last week
29%	"Kosher" observed in household
20%	Recited Kiddush last week
6%	Attended Jewish religious services on all or most Saturdays or Sundays during the past 12 months

(coefficient of reproducibility = .90; coefficient of scalability = .63)

Associational

.45338	Active in a Jewish club or organization
.41879	Officer in a Jewish club or organization
.37049	Active in a temple

Fraternal

.35602	Most friends are Jewish
.34716	Most neighbors are Jewish
.33379	Most business associates are Jewish
.31218	Being Jewish affects choice of where to live

Ideological (Guttman scale)
89% Agrees that U.S. Jews should do all they can to help Israel
56% Agrees that Six-Day-War raised the status of the Jewish people in the eyes of American non-Jews
36% Plan to go to Israel within the next three years
15% Has been to Israel
11% Agrees that U.S. Jews should move to Israel

(coefficient of reproducibility = .91, coefficient of scalability = .61)

Intellectual
.29764 Claims to know Jewish culture
.28539 Claims to know Jewish history
.28062 Claims to know Jewish religious teachings
.27512 Claims to know Jewish ethics

Esthetic
.40949 Enjoys Jewish music
.40352 Enjoys Jewish literature
.38546 Possesses Jewish art objects

Affectional
.30860 Is happy to be Jewish
.30666 Agrees that being Jewish means something very definite to him/her
.28054 Believes in the Jewish religion
.27906 Feels that it is important that there will always be a Jewish people
.19630 Would switch from being Jewish to something else if it could be done easily

Jewish Identification (JI)
.25127 Esthetic
.24576 Intellectual
.24519 Ideological
.23939 Affectional
.23782 Devotional
.20817 Fraternal
.19323 Associational

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The Polls: American Attitudes Toward the Soviet Union and Communism

TOM W. SMITH

DURING the first half of the 1970s détente warmed Soviet-American relations. A series of major treaties from the SALT I accords in 1972 to the Helsinki Agreements in July 1975 raised the promise of peaceful coexistence and normalized relations. Since then Soviet-American relations have chilled in the face of the huge Soviet arms buildup, Russian-Cuban adventurism in Africa, the Soviet invasion of Afghanistan, and Communist repression in Poland, and commentators are talking of a second cold war.[1]

The change in Soviet-American relations can be charted in American public opinion as clearly as in official diplomatic announcements and news stories. From intense dislike of the Russians during the peak of the Cold War of the 1950s, American favorableness toward the Soviets increased until in 1973 a majority of Americans had rather neutral feelings and nearly a fifth liked the Soviets. Since 1973 favorable opinion of the Soviet Union has fallen sharply, reaching a low point in the immediate aftermath of the invasion of Afghanistan. Likewise, negative ratings of Communism as a form of government and concern about Russia and Communism have increased monotonically since the early 1970s. The public has also judged that relations between the United States and the Soviet Union have deteriorated since the early 1970s. The public evaluated President Carter's approach as tending to be too soft, and at least initially has voiced much more satisfaction with President Reagan's harder line toward the Soviets.[2]

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[1] Bruce Russett and Donald R. Deluca, "'Don't Tread on Me': Public Opinion and Foreign Policy in the Eighties," Political Science Quarterly (1981) 96:381–399. Daniel N. Nelson and Pamela Johnston Conover, "The American Public and a 'Second Cold War'," Co-existence (1981) 18:141–61.

[2] Tom W. Smith, "Red in the Morning: Trends in American Attitudes Towards the Soviet Union and Communism," unpublished NORC report, Nov., 1982. John P. Robinson and Robert Meadow, Polls Apart, Cabin John, MD.: Seven Locks Press, 1982.

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The public has also grown increasingly concerned about the balance of power between the Soviets and the Americans. During the 1960s the public thought American power was more likely to increase than Soviet power. The perceived American edge declined, however, and in 1974 Soviet power was rated as more likely to rise than American. This Soviet advantage was seen as persisting throughout the 1970s, but with the margin declining until in 1980 momentum was finally seen as returning to America. Similar trends were indicated by items on power and importance and being a vital force in the world. In the military sphere the public in the late 1970s saw the United States as already inferior to the Soviets and tending to sink further. Some expressions that the United States had begun to redress the balance have begun to appear in the 1980s. Since the American public is firmly committed to the military superiority of the United States, concern about Soviet military strength contributed to a rise in support for defense spending from 1973 to 1978 and a tremendous surge in support following the Afghanistan invasion. By 1982, however, support for more defense spending had fallen back to 1978 levels.[3] In response to the widely publicized (but actually rather modest) increases in the defense share of the federal expenditures, the public no longer sees the military as being drastically shortchanged.

The public has also become more willing to oppose the spread of Communism.[4] In the early 1970s, during the zenith of détente and the aftermath of the Vietnam War, the public was unwilling to oppose Soviet expansionism even while expecting it to continue. From then until 1980, support for the defense of key allies such as Western Europe and Japan rose steadily, and minority backing for military intervention in such areas as Yugoslavia also enlarged. There is some indication, however, that support for military action may have ebbed slightly by 1982, and is still very limited in many areas. For example, in El Salvador the assignment of a small contingent of military advisors is supported by only a little over a third of the public. Many Americans fear a repetition of the Vietnam War and are unwilling to back even token military involvement. Similarly in Poland, support for strong action is limited. When asked to choose among various options, the use of troops to oppose Soviet invasion is endorsed by less than 10 percent of the public, and a quarter or more of the public wants the United States to stay completely out of the confrontation.[5]

The growing unfavorable opinion of the Soviet Union and concern about Soviet goals, actions, and its military might also have led to increased caution about the type of mutual ties and bilateral agreements that were the heart of détente. While still in principle in favor of a host of commercial, cultural, political, and diplomatic ties, growing minorities began to doubt the mutual advantage and actual benefits of such links during the late 1970s and early 1980s. Similar changes have occurred in the crucial area of disarmament. Large majorities of the public have backed the principle of arms control and the reduction of strategic nuclear weapons. The case of SALT II, however, clearly indicates that the public is not willing to endorse any proposed arms settlement. When the outlines of the SALT II treaty were becoming clear and at the time of signing in June 1979, a plurality of informed Americans backed the pact. (But a majority of Americans either were uninformed or had no opinion on the matter.) As opinion crystallized during the summer and early fall, a plurality came to oppose the treaty. This reversal appears to have come from the publicizing of significant Senate opposition to the treaty and the brouhaha over the Soviet combat brigade in Cuba that broke in the early fall of 1979. Recent upsurge in disarmament activity centering around the nuclear freeze movement does not vary from the earlier pattern—strong support for the principle and goal of arms reduction and the hoped-for lessening of the threat of nuclear war, but major requirements that the reductions be equitable and verifiable.

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[3] Everett Carll Ladd, Jr., Marilyn Potter, Linda Basilick, Sally Daniels, and Dana Suszkin, "The Polls: Taxing and Spending," Public Opinion Quarterly (1979) 43:126–35. Louis Kriesberg and Ross Klein, "Changes in Public Support for U.S. Military Spending," Journal of Conflict Resolution (1980) 24:79–111.

[4] John M. Benson, "The Polls: U.S. Military Intervention," Public Opinion Quarterly (1982) 46:592–98; Lloyd Free, and William Watt, "International Comes of Age . . . Again," Public Opinion 3 (April/May, 1980): 46–50; and Russett and Deluca, "Don't tread."

[5] Smith, "Red in the Morning."

On the domestic front some differences show up from the postdétente trends on the international level. American Communists are apparently seen as less dangerous now than in the 1950s and are accorded more civil liberties, but they are still seen as undesirable and are ranked at or near the top of various lists of harmful groups and at about the middle among a list of problems. However, there has been no increase in intolerance towards Communists during the 1970s and early 1980s as concern about Communism grew, dislike of Russia soared, and in general Soviet-American relations deteriorated across the board. This lack of connection was not because Americans' attitudes toward domestic and international Communism were unrelated. There are moderate to substantial associations between being tough on Communism (disliking Russia, opposing Communism as a form of government, and supporting defense spending) and opposing civil liberties for Communists. Despite this connection, the increase in international hostility to Communism did not cause a growth in intolerance of domestic Communists.

As far as the public is concerned, we are neither in a period of détente nor in a cold war period like the 1950s. We are, rather, in an armed truce maintained by a balance of power.

The data were compiled with the assistance of the archival staffs of the Roper Center, University of Connecticut; Louis Harris Data Center, University of North Carolina; and the National Opinion Research Center, University of Chicago. These archives supplied data from the following survey organizations:

ABC/WP—American Broadcasting Corporation/Washington Post
CBS/NYT—Columbia Broadcasting System/New York Times
GALLUP—Gallup Polls/American Institute for Public Opinion
GALLUP-CCFR—Gallup for the Chicago Council on Foreign Relations
GALLUP-POTOMAC ASSOCIATES—Gallup for Potomac Associates
HARRIS—Louis Harris and Associates
HARRIS-CCFR—Harris for Chicago Council on Foreign Relations
LATIMES—Los Angeles Times
NBC/AP—National Broadcasting Company/Associated Press
NORC—National Opinion Research Center, University of Chicago
NORC-GSS—NORC's General Social Survey
RESPONSE ANALYSIS—Response Analysis Corporation
ROPER—Roper Organization
YANKELOVICH—Yankelovich, Skelly, and White

For access to these data the individual organizations or their archives should be contacted. The dates given for the surveys are the months the surveys were fielded. Samples sizes were typically around 1,500.

General Attitudes Toward Communism/Russia

GALLUP/NORC-GSS: You will notice the boxes on this card go from the highest position of "plus 5" for a country which you *like* very much, to a position of "minus 5" for a country you *dislike* very much. How far up the scale or how far down the scale would you rate the following countries?[a]

Russia	*Oct 1953*	*Aug 1954*	*Sep 1954*	*Dec 1956*	*Dec 1966*	*May 1972*	*Apr 1973*[b]	*Jul 1973*	*Mar 1974*	*Mar 1975*	*Jun 1976*	*Mar 1977*	*Feb 1979*	*Jan 1980*[b]	*Mar 1982*
Favorable	0.9%	1.7%	0.4%	1.9%	16.6%	18.9%	15.7%	19.3%	19.1%	18.7%	8.0%	12.4%	13.5%	4%	7.3%
Mixed	10.6	10.8	8.5	11.7	30.0	38.0	43.9	51.0	45.3	45.2	33.9	40.1	37.6	23	31.7
Unfavorable	88.5	87.5	91.1	86.4	63.4	43.1	40.4	29.7	35.6	36.1	58.1	47.5	48.9	73	61.0

[a] NORC-GSS for points in March of 1974, 1975, 1977, and 1982. Wording varies slightly between Gallup and NORC-GSS surveys.

[b] You will notice that the 10 boxes on this card go from the highest position of plus 5—for a country you have a very favorable opinion of—all the way down to the lowest position of minus 5—for a country you have a very unfavorable opinion of. How far up the scale or how far down the scale would you rate the following nations? Soviet Union.

GALLUP-POTOMAC ASSOCIATES: Now I'd like to find out how worried or concerned you are about some of the international problems we face: a great deal, a fair amount, not very much, or not at all. If you really aren't concerned with some of them, don't hesitate to say so.

The problem of the Soviet Union	*1964*	*1972*[a]	*1974*	*1976*
Great deal	—	24%	32%	50%
Fair amount	—	38	36	25
Not very much	—	27	20	18
Not at all	—	6	8	5
Don't know	—	5	4	3
The threat of Communism				
Great deal	—	41%	46%	50%
Fair amount	—	29	21	25
Not very much	—	19	21	18
Not at all	—	7	9	5
Don't know	—	4	3	2
Composite score[b]	86	67	69	74

[a] "The problem of Soviet Russia" and "The threat of communism at home and abroad."

[b] Composite score: great deal = 100, fair amount = 67, not very much = 33, not at all = 0.

NORC-GSS: Thinking about the different kinds of governments in the world today, which of these statements comes closest to how you feel about Communism as a form of government?

	Mar 1973	*Mar 1974*	*Mar 1976*	*Mar 1977*	*Mar 1980*	*Mar 1982*
It's the worst kind of all	44.2%	50.5%	52.3%	53.6%	58.6%	61.0%
It's bad, but no worse than some others	27.5	27.1	26.4	24.9	27.1	25.3
It's all right for some countries	25.1	19.3	20.1	20.1	13.3	12.2
It's a good form of government	3.1	3.1	1.2	1.4	1.1	1.5

ROPER: In your opinion, which of the following best describes Russia's primary objective in world affairs?

	Jun 1978	*Oct-Nov 1979*	*Feb 1980*	*Nov 1980*	*Sep 1981*
Russia seeks only to protect itself against the possibility of attack by other countries	7%	9%	5%	5%	6%
Russia seeks to compete with the U.S. for more influence in different parts of the world	25	30	18	21	18
Russia seeks global domination, but not at the expense of starting a major war	34	37	34	38	35
Russia seeks global domination and will risk a major war to achieve that domination if it can't be achieved by other means	23	18	39	28	34
Don't know	11	6	4	8	7

Soviet/American Relations

HARRIS: Do you feel relations between the United States and Russia are improving, getting worse, or are about what they have been?

	Jul 1968	*Oct 1968*	*Nov 1968*	*Aug 1970*	*Jun 1971*	*Feb[a] 1972*	*May 1972*	*Jun 1973*
Improving	33%	18%	24%	27.9%	37.4%	39.5%	28%	66%
Getting worse	8	14	7	6.5	4.0	6.7	10	2
About the same	52	63	63	57.6	52.2	47.8	57	24
Not sure	7	5	6	7.9	6.4	6.0	5	8

[a] "The same as" replaces "what."

GALLUP-POTOMAC ASSOCIATES: Now, I'm going to mention some of the problems we face in the international field and ask whether from your own point of view the United States has made much progress, some progress, stood still, lost some ground, or lost much ground in handling each of them during the last year or two.

And what about relations with the Soviet Union?

	1972[a]	*1974*	*1976*
Made much progress	10%	10%	4%
Made some progress	59	49	35
Stood still	18	24	34
Lost some ground	3	7	14
Lost much ground	1	2	2
Don't know	9	8	11

[a] Omits "of the," substitutes "12 months" for "year or two," and "Soviet Russia" for "the Soviet Union."

ROPER: There are many problems facing our nation today, all of which the federal government must be concerned with. But at certain times some things are more important than others, and need more attention from our federal government than others [card shown respondent]. I'd like to know for each of the things on this list whether you think it is something the government should be making a *major* effort on now, or something the government should be making *some* effort on now, or something not needing any particular government effort now: Trying to improve relations between the United States and Russia.

	Jun[a] *1974*	*Jun* *1975*	*Jun* *1976*	*Jun*[b] *1977*	*Jun*[b] *1978*	*Jun*[b] *1979*	*Jun*[b] *1980*	*May-Jun*[b] *1981*
Major effort	33%	38%	37%	42%	42%	41%	47%	46%
Some effort	44	44	45	43	40	43	38	39
No particular effort	18	12	13	10	13	11	11	11
Don't know	4	6	6	5	5	5	3	4

[a] Omits "federal" from "our federal government."
[b] Omits "all of which . . . with."

Soviet/American Power and Importance

GALLUP: Which of these do you think is likely to be true in 19__? A year when America will increase her power in the world or a year when American power will decline?

	1960[a]	*1963*	*1965*	*1966*	*1967*	*1968*	*1969*	*1974*	*1976*	*1977*	*1978*	*1979*	*1980*
Increase	72%	84%	64%	74%	66%	63%	62%	29%	42%	58%	42%	53%	58%
Decrease	10	6	19	14	10	22	21	50	44	24	26	32	30
Don't know	18	10	17	12	14	15	17	21	14	18	8	15	12
Same	—	—	—	—	—	—	—	—	—	—	24	—	—

A year when Russia will increase her power in the world, or a year when Russian power will decline?

	1960[a]	*1963*	*1965*	*1966*	*1967*	*1968*	*1969*	*1974*	*1976*	*1977*	*1978*	*1979*	*1980*
Increase	53%	37%	38%	42%	49%	56%	58%	55%	63%	63%	53%	61%	56%
Decrease	23	45	33	36	26	22	19	14	18	16	16	19	22
Don't know	24	18	29	22	25	22	23	31	19	21	11	20	22
Same	—	—	—	—	—	—	—	—	—	—	20	—	—

[a] Year referred to in questions. Surveys are usually conducted in December of the previous year. Introductory sentence varies slightly.

ROPER: Now here is a list of different countries [card shown respondent]. For each one will you tell me whether you think it is likely to be a more vital force in the world 10 years from now, a less vital force, or about the same as it is now?

	Mar–Apr *1974*	*Apr* *1975*	*Mar* *1976*	*Mar* *1979*	*Mar* *1982*[a]
Russia					
More vital	66%	58%	57%	66%	52%
Less vital	4	4	6	4	6
About the same	19	26	25	22	34
Don't know	10	12	13	9	8
United States					
More vital	59	53	49	68	62
Less vital	8	10	11	7	3
About the same	27	29	32	20	30
Don't know	6	8	8	5	5

[a] "Now would you read down the list once more and for each one tell me . . ."

GALLUP-POTOMAC ASSOCIATES: Here is a picture of a ladder. I'd like you to show me on the ladder how much power and importance you feel each of the countries I am going to mention has today. A very great power you consider to be in the very strongest class would be at the top of the ladder; a very small, weak power at the bottom. Naturally, if you think two countries are of equal rank you can put them on the same step.

	Today		*10 Years Ago*		*10 Years Ahead*	
	1974	*1976*	*1974*	*1976*	*1974*	*1976*
United States	8.8	8.5	9.2	8.9	8.0	8.4
Soviet Union	7.8	8.2	—	6.9	7.9	8.6

Military Balance of Power and Defense Spending

CBS/NYT: Right now, would you say the United States is superior in military strength to the Soviet Union, is about equal in strength, or is not as strong as the Soviet Union?

	Jun 1978	*Jun 1979*	*Jan 1980*	*Jan 1981*	*Jan 1982*
USA superior	12%	11%	14%	11%	9%
About equal	32	30	34	39	37
USA inferior	42	43	42	42	44
No opinion	14	16	10	8	10

CBS/NYT: Right now, would you say the United States is superior in nuclear strength to the Soviet Union, or is about equal in strength, or is not as strong as the Soviet Union?

	May 1982
USA superior	9%
About equal	33
USA inferior	47
No opinion	11

ROPER: At the present time do you think the United States is ahead of Russia in terms of nuclear arms capability, about even with them, or behind Russia in nuclear military strength?

	Nov 1977	*Jun 1978*	*Jan 1979*	*Sep–Oct 1980*	*Oct 1981*	*Apr 1982*
U.S. ahead	18%	17%	18%	15%	16%	12%
About even	38	36	38	29	35	32
U.S. behind	28	31	31	43	41	41
Don't know	15	16	12	13	9	15

NBC/AP: As of now, do you think the United States is stronger militarily than Russia, do you think that Russia is stronger than the United States, or are both countries about equal in military strength?

	Jun 1978	*Apr–May 1979*	*Jan 1980a*	*Jan 1980b*	*Jul 1980*[a]	*Feb 1981*	*Sep 1981*	*Dec 1981*[b]	*Mar 1982*[b]
U.S. stronger	17%	17%	15%	12%	6%	10%	14%	13%	14%
Russia stronger	37	37	40	47	58	49	36	38	38
About equal	35	39	38	32	28	33	41	39	38
Not sure	11	7	7	9	8	8	9	10	10

[a] Likely voters.

[b] "Soviet Union" instead of "Russia."

CBS/NYT: Do you think the military strength of the United States should be superior to the Soviet Union, should be about equal in strength, or doesn't the United States need to be exactly as strong as the Soviet Union?

	Jun 1978[a]	*Jun 1979*	*Aug 1980*	*Sep 1980*[b]	*Sep 1980*[b]	*Oct 1980*[b]	*Feb 1981*
USA superior	46%	42%	51%	51%	61%	56%	52%
About equal	47	45	42	41	32	35	39
Doesn't need to be as strong	2	7	2	4	4	5	4
No opinion	5	6	5	4	3	4	5

[a] Omits "exactly."
[b] Registered voters.

NBC/AP: Do you think that the United States *should be* stronger in military terms than Russia, about equal in military strength, or not as strong?

	Jun 1978	*Apr–May 1979*[a]	*Jan 1980a*	*Jan 1980b*
Stronger	52%	57%	67%	64%
Equal strength	39	38	29	31
Not as strong	4	2	2	3
Not sure	5	3	2	2

[a] Substitutes "militarily" for "in military terms."

ROPER: Do you think it is necessary that the United States be stronger than Russia in conventional non-nuclear arms capability, or just as strong as Russia, or doesn't the United States need to be as strong as Russia in *conventional* arms as long as it has enough *nuclear* arms to knock Russia out?

	Feb 1981
Stronger	47%
Just as strong	42
Doesn't need to be as strong	7
Don't know	4

NORC-GSS/ROPER:[a] We are faced with many problems in this country, none of which can be solved easily or inexpensively. I'm going to name some of these problems, and for each one I'd like you to tell me whether you think we're spending too much money on it, too little money, or about the right amount: The military, armaments and defense.

	Jul 1971	*Mar 1973*	*Dec 1973*	*Mar 1974*	*Mar 1975*	*Mar 1976*	*Mar 1977*	*Mar 1978*	*Mar 1980*	*Mar 1982*
Too little	18.1%	11.9%	17.0%	18.1%	17.8%	25.8%	25.2%	29.2%	60.2%	30.9%
About right	40.2	47.8	47.7	48.6	49.0	45.0	48.5	47.1	27.5	37.6%
Too much	41.7	40.3	35.3	33.3	33.2	29.2	26.2	23.6	12.3	31.6

[a] Roper surveys for 1971 and December 1973.

Soviet/American Agreements

ROPER: The United States has formed ties of varying degrees with different nations in the world. Here is a list of a few countries [card shown respondent]. Would you read down that list and tell me for each country what you think would be best for us in the long run—to strengthen our ties with them, or to continue things about as they are, or to lessen our commitments to them?

Russia	*Jan 1974*	*Jan 1975*	*Jan 1977*	*Jan 1981*
Strengthen ties	33%	36%	36%	30%
Continue as now	29	31	28	29
Lessen commitments	25	22	22	30
Don't know	13	11	14	11

NBC/AP: For the past few years, the United States and Russia have had a policy of trying to reach agreements which will relax tensions between them. Do you think the Russians can be trusted to live up to such agreements or don't you think so?

	Aug 1978	*Oct 1978*	*Mar 1979*	*Sep 1979*
Can be trusted	21%	24%	26%	21%
Cannot be trusted	69	66	64	71
Not sure	10	10	10	8

HARRIS: Do you think it is possible for the United States and Russia to reach long-term agreements to help keep the peace or do you think (that) this is not possible?

	1968[a]	*Aug 1970*[b]	*Jun 1971*[c]	*Feb 1972*[c]	*Jun 1973*	*Nov 1973*	*Dec 1974*[d]	*Dec 1975*[e]
Can reach agreements	34%	51.8%	53.6%	55.6%	59	69.2%	59.1%	44.9%
Not possible	50	32.2	33.4	33.9	22	19.7	26.1	38.8
Not sure	16	16.1	13.0	10.5	19	11.1	14.8	16.3

[a] Do you feel the U.S. (United States) and Russia can reach long-term agreements to help keep the peace, or do you think that is not possible?

[b] Do you think it is possible for the United States and Russia to come to a long-term agreement to control wars in the world which will work, or do yo feel it is not possible for that to happen?

[c] Do you think it is possible for the United States (U.S.) and Russia to come to a long-term agreement in the world which will work, or do you feel it is not possible for that to happen?

[d] Substitutes "keep peace" for "help keep the peace."

[e] Russia one country on list. Omits "help."

HARRIS: Would you favor or oppose agreement between the U.S. (United States) and Russia on [read list]?

	Aug 1970	*Jun 1971*	*Feb 1972*
Limiting anti-missile (ABM) systems			
Favor	69.0%	71.5%	73.6%
Oppose	15.9	13.7	13.0
Not sure	15.1	14.8	13.4
Exploring outer space			
Favor	62.0	63.8	66.5
Oppose	27.3	24.8	22.8
Not sure	10.8	11.5	10.7
Expanding trade between the two countries			
Favor	74.6	75.4	75.1
Oppose	13.8	13.8	12.4
Not sure	11.6	10.8	12.6
Exchanging scholars and cultural groups			
Favor	79.5	79.7	82.3
Oppose	11.0	10.8	9.0
Not sure	9.5	9.5	8.8
Taking joint action if another nation threatens to use nuclear weapons			
Favor	74.1	67.5[a]	68.7[a]
Oppose	12.1	14.8	14.2
Not sure	13.8	17.7	17.1
Joint action to keep Communist China from starting wars			
Favor	77.5	69.4	—
Oppose	10.4	14.0	—
Not sure	12.1	16.3	—

Getting European countries to pledge not to go to war against each other			
Favor	78.8	78.2	80.7
Oppose	8.4	9.6	7.3
Not sure	12.6	12.2	12.0
Joint exploration of the oceans			
Favor	70.2	70.5	73.6
Oppose	15.4	14.4	11.9
Not sure	14.4	15.2	14.4
Bringing about a settlement in the Middle East			
Favor	—	81.5	82.2
Oppose	—	8.4	7.2
Not sure	—	10.1	10.6
Reducing the number of American troops in Western Europe			
Favor	—	70.6	76.4
Oppose	—	16.9	8.7
Not sure	—	13.6	15.0

[a] Omits "joint."

HARRIS: Now let me read some proposals which have been made for possible agreements between (Russia and the United States/the United States and the Soviet Union). For each, tell me if you would favor or oppose such an agreement [read list and record below].

	Nov 1973	*Dec 1974*	*Dec 1975*
Expanding trade between the U.S. and Russia			
Favor	71.9%	68.2%[a]	—
Oppose	16.1	20.9	—
Not sure	12.0	10.9	—
Give Russia favored nation status in trade as most Western nations have with us now			
Favor	49.1	62.9[b]	56.4%[c]
Oppose	32.1	23.6	23.2
Not sure	18.8	13.5	20.4
Undertaking joint efforts to curb air and water pollution			
Favor	88.6	81.2	75.2[d]
Oppose	5.4	11.7	9.0
Not sure	6.0	7.1	15.9
Undertaking joint efforts to solve the world energy shortage			
Favor	89.8	82.3	—
Oppose	4.9	9.0	—
Not sure	5.3	8.7	—
Bringing about a Middle East settlement			
Favor	90.7	84.2[e]	77.6[f]
Oppose	3.1	9.3	6.8
Not sure	6.2	6.5	15.6
Reducing the number of American and Russian troops in central and western Europe			
Favor	75.1	72.6[g]	—
Oppose	11.2	12.3	—
Not sure	13.7	15.1	—

Exchanging scientists and other technical missions			
Favor	73.4	64.8	51.3[h]
Oppose	13.8	23.9	27.7
Not sure	12.8	11.3	21.0
Undertaking joint space missions			
Favor	—	82.3	46.0[i]
Oppose	—	9.0	33.8
Not sure	—	8.7	20.2

[a] Substitutes "Soviet Union" for "Russia."
[b] Giving the Soviet Union the same trade treatment that we give other countries.
[c] Give the Soviet Union the same trade treatment that we give other countries.
[d] "Undertake" instead of "undertaking."
[e] Bringing about peace in the Middle East.
[f] Bring about peace in the Middle East.
[g] Reducing the number of American and Russian troops in Europe.
[h] Exchange scientists and other technical missions.
[i] Undertake more joint space missions.

Arms Control and SALT

NBC/AP: Do you favor or oppose a new agreement between the United States and Russia which would limit nuclear weapons?

	Jan 1978	*Jun 1978*	*Aug 1978*	*Oct 1978*	*Nov 1978*[a]	*Feb 1979*	*Mar 1979*	*Apr 1979*	*Jul 1979*	*Sep 1979*	*Oct 1980*
Favor	74%	67%	71%	70%	71%	81%	71%	68%	65%	62%	70%
Oppose	19	22	22	21	17	14	18	22	25	30	21
Not sure	7	11	7	9	12	5	11	10	10	8	9

[a] Voters.
[b] "Do you favor or oppose agreements between the United States and Russia which limit nuclear weapons?

NBC/AP: Do you think the United States should move towards nuclear disarmament on its own . . . only if the Soviet Union agrees to disarm as well . . . or don't you think the United States should move towards nuclear disarmament at all?

	Dec 1981	*Mar 1982*	*May 1982*
Disarm on own	12%	14%	14%
Disarm along with Soviet Union	51	50	50
Not disarm	27	26	28
Not sure	10	10	8

ROPER: The United States and Russia are trying to come to a new agreement limiting each country's nuclear weapons. This agreement would replace the Strategic Arms Limitation Treaty, called SALT, that ran out last October. Are you in favor of, or opposed to signing a new SALT agreement with Russia, or haven't you been paying much attention to this issue?

	Nov 1977	*Jun 1978*	*Aug 1978*
In favor of	43%	45%	41%
Opposed to	10	11	16
Not paying attention	29	30	29
It depends (vol.)	8	6	8
Don't know	10	8	6

In June of 1979, President Carter for the U.S. and President Brezhnev for Russia signed a new SALT treaty. The treaty, which would last until 1985, limits each country to a maximum of 2,250 long-range nuclear missiles and bombers. As you know, there's a good deal of controversy about this proposed treaty. Do you think the U.S. Senate should vote for this new SALT treaty or against it?

	Oct–Nov 1978[a]	*Jan 1979*[a]	*Apr–May 1979*[a]	*Jul 1979*	*Sep 1979*	*Oct–Nov 1979*	*Jan 1980*	*Nov 1980*
For	42%	40%	33%	31%	30%	30%	22%	26%
Against	20	21	24	29	39	35	42	36
Mixed feelings (vol.)	17	19	20	21	15	19	17	20
Don't know	20	20	23	19	17	17	18	19

[a] The U.S. and Russian negotiators have about reached agreement on a SALT treaty. The treaty, which would last until 1985, limits each country to a maximum of 2,250 long-range nuclear missiles and bombers. As you know, there's a good deal of controversy about this proposed treaty. Do you think the U.S. Senate should vote for this new SALT treaty or against it?

LATIMES: Do you approve or disapprove of the SALT II nuclear weapons agreement?

	Nov 1980	*Apr 1981*
Approve	34%	35%
Disapprove	38	36
Not sure	27	27
Refused	1	2

HARRIS: Now let me read you some statements that some people have made about the U.S. and Russia coming to a new SALT arms agreement. For each, tell me if you agree or disagree.

	Jan 1979	*Apr 1979*	*Oct 1979*	*Nov 1980*
At a time when (it is) it's possible for the U.S and Russia to blow each other up with nuclear weapons, it is vital for the two countries to reach an agreement to limit nuclear arms				
Agree	86.2%	86.0%	86.8%	83.9%
Disagree	10.0	10.9	10.3	13.7
Not sure	3.7	3.2	3.1	2.4
Because the chances are that we will keep our end of the bargain and the Russians will not, we should not sign an agreement (with them) limiting nuclear weapons				
Agree	41.5	42.2	49.3[a]	47.3
Disagree	50.2	50.7	40.5	47.7
Not sure	8.4	7.2	10.2	5.1
If our military and Defense Department leaders say the SALT agreement with the Russians is a good deal for us, then the U.S. Senate should ratify such an agreement				
Agree	67.0		70.2[b]	—
Disagree	21.8		19.8	—
Not sure	11.3		10.0	—
Refusal by the U.S. Senate to ratify a new SALT arms agreement could lead to a serious nuclear arms build-up both in this				

country and Russia which could bring us dangerously close to a nuclear war				
Agree	—	66.5	54.7	51.2[c]
Disagree	—	25.0	37.5	42.8
Not sure	—	8.5	7.8	5.9

[a] Because the chances are that we will keep our end of the bargain and the Russians will not, the U.S. Senate should not ratify the SALT treaty President Carter signed in Vienna last June.

[b] The U.S. Senate should ratify the SALT treaty, if our military and Defense Department leaders say that the U.S. has adequate inspection devices to monitor a Russian nuclear arms buildup.

[c] Refusal by the U.S. to agree to a new SALT arms agreement. . . . .

Domestic Communism

YANKELOVICH: How great a danger do you feel that American Communists are to this country at the present time—a great danger, some danger, hardly any danger, or no danger at all?

	Mar 1974	*Sep 1974*	*May 1975*	*Jan 1976*	*Apr 1976*[a]
A great danger	28.6%	29.3%	29.5%	30.0%	26.6%
Some danger	35.6	37.0	31.9	38.0	41.2
Hardly any	19.6	18.3	19.6	17.6	19.6
No danger	11.9	10.6	14.1	9.3	12.6
Don't know	4.3	4.9	5.0	5.0	—

[a] Registered voters.

NORC/AIPO: How great a danger do you feel American Communists are to this country at the present time—a very great danger, a great danger, some danger, hardly any danger, or no danger?

	Summer 1954
Very great danger	19%
Great danger	24
Some danger	38
Hardly any danger	9
No danger	2
Don't know	8

GALLUP: How much danger do you think the Communists right here in America are to this country at the present time—a very great deal, a good deal, not very much, or none at all?

	Sep 1964
Very great deal	13.6%
Good deal	63.3
Not very much	18.9
None at all	0.9
Don't know	3.4

HARRIS: Now let ask you about some specific groups of people in this country. For each tell me if you feel they should or should not be put under surveillance by any government authority.

	% Should	
	Mar 1974	*Jan 1976*
1. American Communist Party	74.9%	75.6%
2. John Birch Society	47.2	46.5
3. Vigilante groups like the Ku Klux Klan (KKK)	74.7	73.4
4. Americans for Democratic Action (ADA)	33.1	31.2
5. American Civil Liberties Union (ACLU)	30.6	30.5
6. National Association for the Advancement of Colored People (NAACP)	36.5	33.8
7. Ralph Nader's Consumer Action group	24.2	26.4
8. Common Cause	17.8	19.5
9. Young Americans for Freedom (YAF)	30.4	27.0
10. Welfare Rights Organization	39.9	39.1
11. The Democratic Party	35.2	31.9
12. The Republican Party	37.0	32.3
13. Students for a Democratic Society (SDS)	39.3	36.8
14. Congressmen who support radical causes	47.1	46.7

NORC/GALLUP/NORC-GSS/RESPONSE ANALYSIS:[a] Now, I should like to ask you some questions about a man who admits he is a Communist. Suppose this admitted Communist wanted to make a speech in your community. Should he be allowed to speak, or not?

	Sum. 1954	*Mar 1972*	*Mar 1973*	*Mar-Apr 1973*	*Mar 1974*	*Mar 1976*	*Mar 1977*	*May 1978*	*May 1980*	*Mar 1982*
Allowed	28.2%	53.7%	61.2%	53%[b]	59.6%	55.8%	56.6%	62.3%	56.6%	57.5%
Not allowed	71.8	46.3	38.8		40.4	44.2	43.4	37.5	43.4	42.5

Suppose he is teaching in a college. Should he be fired?

Fired	93.6	65.4	58.5		55.6	56.4	59.5	—	56.8	53.8
Not fired	6.4	34.6	41.5	30[b]	44.4	43.6	40.5	—	43.2	46.2

Suppose he wrote a book which is in your public library. Somebody in your community suggests that the book should be removed from the library. Would you favor removing it or not?

Favor removing	71.1	44.5	40.1	—	39.3	41.8	43.3	35.9	40.5	40.6
Not favor	28.9	55.5	59.1	—	60.7	58.2	56.7	64.1	59.5	59.4

[a] The 1954 survey was jointly conducted by GALLUP and NORC, the March–April 1973 survey was by Response Analysis, and the 1978 survey by NORC. The remaining points are NORC-GSS.

[b] Don't knows are included in the base.

RESPONSE ANALYSIS: Here is a list of topics which have been discussed in the papers recently. Which ones do you remember talking about with your friends or relatives in the last few weeks or so? Do you think it represents a serious or moderate threat to the United States, or is not a threat to the United States?

	Mar–Apr 1973			
	Serious Threat	*Moderate Threat*	*Not a Threat*	*Don't Know*
Drugs	78%	15%	4%	3%
Crime	75	19	3	3
High prices	75	18	5	6
High taxes	58	27	9	6
Pollution	51	31	11	7
Juvenile delinquency	39	38	16	7
Poverty in the United States	39	36	17	8
Racial problems	33	43	17	7
Communists in the United States	29	33	26	13
Urban problems	25	36	23	16
Population increase	22	35	34	9
Hippies	15	26	51	8
Danger of World War III	14	35	37	13
Radical college students	10	32	46	12
Revolution	10	29	48	14
Right-wing groups	8	27	45	20
Women's liberation	6	18	65	11

NORC: Here is a list of groups in politics. As I read the list, please follow along: Socialists, Fascists, Communists, Ku Klux Klan, John Birch Society, Black Panthers, Symbionese Liberation Army, Atheists, pro-abortionists, and anti-abortionists. Which of these groups do you like the least, or if there is some group that you like even less than the groups listed here, please tell me the name of that group.

	May 1978 *% Least Liked*
Socialists	1%
Fascists	5
Communists	29
KKK	24
John Birch Society	1
Black Panthers	6
Symbionese Liberation Army	8
Atheists	8
Pro-abortionists	4
Anti-abortionists	2
Other	2
Don't know	10

YANKELOVICH: How do you feel about some of the organizations which take stands on these and other issues? Do you feel that [name of organization] is good for the country or not that good for the country; or aren't you familiar enough to say?

	Jan 1981
National Rifle Association	
Good for country	31.6%
Not good for country	22.3
Not familiar	46.1
National Organization for Women (NOW)	
Good for country	52.4
Not good for country	17.7
Not familiar	29.9
The National Association for the Advancement of Colored People (NAACP)	
Good for country	70.8
Not good for country	12.3
Not familiar	16.9
The Communist Party	
Good for country	8.1
Not good for country	81.6
Not familiar	10.2
American Civil Liberties Union	
Good for country	41.8
Not good for country	21.4
Not familiar	36.8
Nader's Raiders	
Good for country	41.8
Not good for country	21.4
Not familiar	36.8
Moral Majority	
Good for country	29.4
Not good for country	26.4
Not familiar	44.2
Nic-Pac	
Good for country	2.7
Not good for country	6.1
Not familiar	91.2
Ku Klux Klan	
Good for country	2.7
Not good for country	91.1
Not familiar	6.2

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Why Does Presidential Popularity Decline? A Test of the Expectation/Disillusion Theory

LEE SIGELMAN AND KATHLEEN KNIGHT

DURING the first few months of his term a president can seemingly do no wrong in the eyes of the American public, but before long the "honeymoon" ends and the president's popular appeal begins to wane. Sometime during the third year, presidential popularity bottoms out, after which it may rebound somewhat but never fully recovers (Mueller, 1973; Stimson, 1976). This sequence of events has been played out so many times during the postwar era that it is now considered virtually inevitable; in John Mueller's (1973: 233) words, "If a president wants to leave office a popular man, he should either (1) be Dwight David Eisenhower, or (2) resign the day after inauguration."

It is one thing to know *that* a presidential unpopularity cycle exists, but it is something else to know *why* it exists. Despite the appearance during the last decade of several dozen studies of the dynamics of presidential popularity, we still know remarkably little about why the popularity curve slopes downwards with such regularity. The purpose

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Abstract According to the "expectation/disillusion" interpretation of the decline of presidential popularity over time, popularity declines as unrealistically high expectations of presidential performance inevitably give way to more realistic assessments. This paper puts that interpretation and several specific aspects of it to the test through analysis of data from a series of nationwide polls conducted between 1977 and 1979.

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of this paper is to test what seems to us to be the most plausible explanation that has yet been offered, the so-called "expectation/disillusion theory" (Stimson, 1976).

Explaining the Growth of Presidential Unpopularity

What accounts for the large-scale erosion in public support that has beset each postwar presidency except Eisenhower's and the prematurely terminated Kennedy's? Mueller, adapting one of Anthony Downs's (1957) ideas, sought the answer in an antipresidential "coalition of minorities." According to this explanation, the longer a president stays in office, the more decisions he makes, and the more decisions he makes, the more people he antagonizes. Even if each decision gains widespread acceptance, each is also likely to displease at least a few people. "A clever opposition, under appropriate circumstances, could therefore form a coalition of these intense minorities until it had enough votes to overthrow the incumbent" (Mueller, 1973: 205).

In our view, this explanation falls short in several respects. In the first place, a coalition of minorities cannot form without being abetted by a "clever," well-organized opposition, and in recent decades the out-party has certainly not always been well-organized, let alone clever. Irrespective of the quality of the opposition, a coalition of minorities should emerge only under "appropriate" circumstances. Mueller did not indicate what these circumstances might be, but at the very least they ought to include a public that is well informed and highly issue-oriented, along with a president who is either sufficiently willing or sufficiently inept to make an incessant series of decisions which alienate different segments of his support coalition. There is also reason to believe that it is not presidential decisions per se but the *results* of these decisions which affect the president's public standing (Kernell, 1978:521). If this is true, then an explanation which focuses on presidential decision making seems somewhat misdirected.

According to a second explanation, length of time in office has no substantive meaning of its own, but simply provides the *context* within which various phenomena, such as economic fluctuations, wars, and international rally points, occur. It is these time-based phenomena and not time itself which influence presidential popularity (Kernell, 1978). Consistent with this view, Kernell (1978:520) reports that when the effects of various time-based phenomena are held constant, the passage of time no longer has any impact on presidential popularity. However, Kernell's argument does not explain why popularity almost always *declines* over time. The only answer that

Kernell's interpretation seems to permit is that substantive time-based phenomena (i.e., occurrences in the domestic and international arenas) invariably begin to deteriorate very early in the president's term and continue to do so until some point in the third year—not, we think, a very realistic assumption.

The real problem is to determine why the reaction to the substance of presidential actions nearly always has a cumulatively negative impact on presidential popularity. Mueller's "coalition of minorities" interpretation provides an answer, but one which addresses the individual-level opinion dynamics underlying aggregate popularity change in a very oblique fashion. This brings us to an alternative interpretation which has been most clearly articulated by James Stimson (1976) but which, as Stimson acknowledged, is derived from Mueller's *War, Presidents and Public Opinion*. The basic idea underlying this interpretation is that "in the process of being elected, the president invariably says or implies that he will do more than he can do, and disaffection of once bemused supporters is all but inevitable" (Mueller, 1973:206).

In contrast to the "coalition of minorities" interpretation, which places the blame squarely on the president's shoulders, Stimson (1976) argued that the erosion of presidential popularity is impervious to presidential initiatives and is best understood as a result of the inevitable disjuncture between presidential promise and performance. Stimson assumed, on the basis of considerable evidence, that many Americans are poorly informed and have no well-developed policy preferences. In these circumstances, the "utter simplicity" of campaign pledges contributes to the spread of naive expectations about what a candidate will be able to accomplish if he is elected. In Stimson's words:

> This juxtaposition of mass expectations of results with mass inattention to policy bodes ill for new presidents. It gives them popular mandates to achieve their promised ends (the result of public policy), but no commitment to the means (the policy itself). . . . All these factors conspire to produce unrealistic early expectations and ensure the inevitability of later disillusionment. Because the public, and particularly the ill-informed, expects more than can possibly be achieved under the best of circumstances, it is always prone to great disappointment over what is actually achieved by merely mortal presidents acting under less than ideal conditions. Early naive expectations lead to later cynicism (Stimson, 1976:9–10).

So while the "coalition of minorities" explanation portrays the president as the architect of his own ultimate comeuppance, Stimson (1976:10) sees the president as "largely a passive observer of his downsliding popularity," the causes of which "lie more in the misperceptions of the electorate than in the president's actions." Unrealistic early expectations about what the president will accomplish (or what might be called a "willing suspension of disbelief") give way to disillusionment (disbelief) and ultimately to a decline in popularity.

Stimson invoked the "expectation/disillusion theory" in a post hoc attempt to account for the aggregate opinion dynamics he had observed in the Gallup data on presidential popularity. These data allowed him to present an elegant model of the presidential unpopularity cycle, but did not permit him to test the expectation/disillusion interpretation. Later, Presser and Converse (1976–1977) employed Gallup data to put one part of Stimson's interpretation to the test. Their test focused on Stimson's contention that less sophisticated people are especially likely to hold unrealistic initial expectations of the president. If this is true, then over time the less sophisticated should be susceptible to sharper declines in presidential support than their more sophisticated counterparts, whose initial expectations were more realistic. Presser and Converse tested this proposition by disaggregating the Johnson, Nixon, and Ford popularity trends according to levels of education, the best available measure of sophistication. Their observations did not bear out the proposition, for over time presidential popularity declined more or less equally across levels of education. Taking the analysis a step further, they also noted substantial differences in the rate of decline in presidential approval as a function of party identification: presidential popularity curved downwards among Democrats, Republicans, and independents alike, but the slope was considerably steeper among out-party than in-party identifiers.

In a rejoinder to the Presser-Converse study, Stimson (1976–1977) observes that the data sets used in his analysis and in the Presser-Converse follow-up contain no measure of expectations concerning what the president will achieve, the central concept in his explanation of the presidential unpopularity cycle. He goes on to argue, drawing on other evidence, that presidents who have taken office surrounded by the grandest expectations about their policy agendas have subsequently suffered the severest popularity declines. This new evidence does not, he concludes, invalidate Presser and Converse's findings, but it does restore "a margin of credibility" to the expectation/disillusion theory (Stimson, 1976–1977:543).

Stimson is quite correct when he points out that Presser and Converse's findings, though not entirely consistent with his argument, are not devastating, because data concerning *expectations* are nowhere to be found in the Presser-Converse analysis. (Of course, the same can also be said of Stimson's own analysis.) The absence of data on expectations becomes all the more pressing when it is recognized that the expectation/disillusion theory does not anticipate any *direct* impact of sophistication upon presidential popularity. Rather, what the theory predicts is, first, that lack of sophistication should lead to unrealistically high expectations of the president, and, second, that disillusionment with the president should follow on the heels of high expectations. Given the contingent nature of both links in the explanatory chain, it does not necessarily follow from Stimson's argument that there should be any differential trends in presidential popularity based on level of education (the measure of sophistication).

The ideal way to test the expectation/disillusion theory would be to use data from a panel study in order to probe the individual-level relationships between expectations and evaluations of the president, and between expectations and sophistication. To the best of our knowledge, such panel data are not presently available, nor are they likely to become available within the foreseeable future. Stimson's (1976–1977) attempt to gauge the impact of expectations upon subsequent presidential popularity is a valiant effort to do what can be done with available data, but it is vitiated by the unavailability of *individual-level* data on both expectations and presidential approval. The presidents who entered office amidst the highest expectations may well have suffered the steepest popularity declines, as Stimson contends, but it cannot be legitimately inferred that the people who initially expected the most were the ones who subsequently turned against the president. Thus, while we know on the basis of Stimson's evidence as well as more definitive evidence presented elsewhere (Public Opinion, 1981:39) that recent presidents have taken office in an atmosphere of heady expectations about what they would be able to accomplish, we do *not* know that presidential popularity declines as a function of the dawning realization that such expectations will not be borne out.

In sum, aggregate data from a series of cross-sectional surveys, which are readily available, do not really permit one to test the expectation/disillusion theory. On the other hand, individual-level data from several waves of a panel, which would permit such a test, are not available and are unlikely to become so. Where does this leave us? We believe that the best available means of testing the expectation/disillusion theory is to analyze individual-level data from a series of cross-sectional surveys—assuming, of course, that these surveys contain data on expectations of the president. With such data in hand, one can test a mixed set of longitudinal and cross-sectional propositions which collectively comprise the central elements of the expectation/disillusion interpretation. These propositions are as follows:

(1) Over time, there should be an erosion in public expectations about what the president will achieve *and* in the level of presidential popularity.

(2) At any given point in time, there should be a strong positive relationship between expectations of the president and approval of the president.

(3) Early in a president's term, there should be a sizable gap between those who are less sophisticated and those who are more so in terms of their expectations about what the president will be able to accomplish; the more sophisticated should have more realistic (lower) initial expectations.

(4) As time passes, this gap should narrow considerably, as the less sophisticated become more cynical.

The second proposition merits special comment, for if it does not hold, then nothing else in Stimson's interpretation follows. Assuming, contrary to the proposition, that there were no correlation between expectations and approval, then we would have no reason to expect popularity to erode when expectations decline. On the other hand, if there were an *inverse* correlation between expectations and approval, then we would have to predict that the president would become more popular as expectations about what he will accomplish declined—a prediction that makes no sense. Thus the second proposition, in combination with the first, actually constitutes the core of the expectation/disillusion theory, which reduces to the idea that because there is a positive relationship between expectations and popularity, what causes the latter to erode over time is a decline in the former.

Testing the Expectation/Disillusion Thesis

These propositions cannot be tested via analysis of Gallup opinion data, which have served as the data source for most prior examinations of trends in presidential popularity (including the Stimson (1976) and Presser (1976–77) studies), for the simple reason that the Gallup Poll does not make use of questions concerning *expectations* about what the president will accomplish. In the early part of the Carter administration, however, the CBS News/*New York Times* poll did employ a set of questions concerning the prospects for President Carter to carry out his campaign promises—questions which were used intermittently over the first two-and-a-half years that Carter was in office. The expectation question read as follows:

Here are some of the things Jimmy Carter said he would like to do as president. Please tell me whether you think he will, or will not, be able to do them.

Do you think Jimmy Carter will or will not be able to [read each item] . . .

restore trust in government?
streamline the government and make it more efficient?
balance the federal budget within the next four years?
keep prices from going up all the time?
reduce unemployment to any real extent?
handle the energy problems effectively?
bring peace to the Middle East?

Unfortunately, the expectation question was not used over the entire course of the Carter presidency, nor were all the items repeated each time the question was asked. However, the question was used often enough to make it possible to trace the connection between expectations and popularity over time, and to see whether early expectations of President Carter were really as exaggerated as the expectation/disillusion theory suggests.

In the same surveys, respondents were also asked the CBS News/*New York Times* poll presidential popularity question, which is identical to the Gallup question employed in the Stimson (1976) and Presser-Converse (1976–1977) studies: "Do you approve or disapprove of the way Jimmy Carter is handling his job as president?" Following Stimson and Presser and Converse, we use "relative approval" (percentage approving as a proportion of those expressing opinions) as the measure of presidential popularity.

POPULARITY AND EXPECTATION TRENDS

Table 1 reports the relative approval score for Carter at the six time points for which expectation data are available, beginning in April, 1977, and ending in June, 1979. During the first six months of the Carter administration, more than three out of every four respondents who held opinions one way or the other expressed approval of the new president's performance in office. After that, however, Carter's popularity began to slide. Largely on the basis of this massive erosion of support, Carter took to the airwaves in July, 1979, to proclaim a "crisis of confidence" which threatened the nation's very survival.

The first row of Table 1 merely reconfirms what should not need reconfirmation: that Jimmy Carter's popularity underwent a drastic decline during his first 30 months in office. What we need to determine at this point, as best we can, is whether expectations about what Carter would be able to accomplish underwent a parallel decline. If

Table 1. Presidential Popularity and Expectations over Time

	Apr. 1977	*July 1977*	*Oct. 1977*	*Jan. 1978*	*Jan. 1979*	*June 1979*
Approval of Carter	76.5%	78.1%	65.8%	63.9%	49.1%	32.8%
Agreement that Carter would:						
Restore trust in government	76.9	69.5	56.8	58.1	38.0	28.9
Streamline the government and make it more efficient	59.2	55.3	45.8	39.0	NA	NA
Balance the federal budget within the next four years	25.4	23.0	19.6	15.8	17.7	NA
Reduce unemployment to any real extent	51.2	41.9	37.1	34.0	NA	NA
Keep prices from going up all the time	17.3	16.2	NA	12.3	NA	NA
Handle the energy problems effectively	55.6	45.8	46.3	43.2	NA	NA
Bring peace to the Middle East	32.9	34.3	32.6	39.5	NA	NA

NOTE: NA = not available.

they did, then the expectation/disillusion theory will be worth pursuing; if they did not, then our test of the expectation/disillusion thesis need go no farther.

The rest of the data presented in Table 1 make it clear that during President Carter's first two-and-a-half years in office, expectations about what he would be able to accomplish and evaluations of his performance did follow a parallel course. Carter's popularity was extremely high shortly after his inauguration but declined sharply thereafter. The same can be said of expectations regarding the symbolic issues central to the 1976 campaign—that Carter would restore trust in government and make it run more efficiently. As attention turned to more concrete issues, however, initial expectations appear to have become tempered by an element of "realism." Expectations about what Carter would accomplish economically were fairly pessimistic at the outset but became even more pessimistic as time wore on. More was expected in terms of Carter's ability to cope with energy issues, but here too expectations eroded over time. Only on the issue of peace in the Middle East did expectations of what Carter would accomplish show any resiliency; but unfortunately for Carter, foreign affairs do not become highly salient to most Americans until the United States becomes actively involved in armed conflict (Mueller, 1973).

THE RELATIONSHIP BETWEEN EXPECTATIONS AND POPULARITY

Now that the expectation/disillusion theory has passed its first test, we must move inside these aggregate expectation and popularity trend lines in order to determine, at the individual level, whether there was any connection between expectations and evaluations of presidential performance. Table 2 presents the correlations between the seven expectation items and presidential approval—correlations that are relatively invariant across columns. In the absence of any change over time, we can say that throughout Carter's first two and a half years in office those who expected him to accomplish the most also gave him the highest marks for his performance. The relationship is somewhat stronger for the "symbolic" expectation items than for the more concrete policy issues, but only for the expectation that Carter would "bring peace to the Middle East" is this difference appreciable—further evidence, we think, that foreign affairs are normally not a very salient basis for political evaluation. This one slight deviation should not, however, obscure the main message of Table 2, which is that at the individual level approval and expectations are closely tied to one another. This finding is not unexpected—indeed, it would have been surprising had we uncovered no relationship at all, and astonishing had we uncovered an inverse correlation—but it is very important nonetheless. In combination with the finding that aggregate levels of popularity and expectations declined together over time, the individual-level correlation between expectations and presidential approval means that the expectation and popularity data are wholly consistent with the predictions we have derived from the expectation/disillusion theory.

THE IMPACT OF SOPHISTICATION

This is not to say, however, that all of our predictions have been borne out, for there remains the issue—central in Presser and Con-

Table 2. Correlations Between Expectations and Presidential Approval

	Correlation with Presidential Approval					
Expectation	*Apr. 1977*	*July 1977*	*Oct. 1977*	*Jan. 1978*	*Jan. 1979*	*June 1979*
Restore trust in government	.758	.788	.773	.625	.772	.809
Streamline the government and make it more efficient	.739	.776	.791	.607	NA	NA
Balance the federal budget within the next four years	.724	.626	.726	.647	.687	NA
Reduce unemployment to any real extent	.557	.691	.713	.469	NA	NA
Keep prices from going up all the time	.538	.450	NA	.448	NA	NA
Handle the energy problems effectively	.745	.749	.760	.571	NA	NA
Bring peace to the Middle East	.365	.635	.493	.447	NA	NA

NOTE: The correlation coefficient is gamma. NA = not available.

verse's extension of Stimson's analysis—of the impact of sophistication upon expectations. If exaggerated expectations about what the president will be able to accomplish reflect a lack of sophistication, then we should find inverse correlations at the beginning of the presidential term between optimism and level of education, Presser and Converse's surrogate for sophistication. Because the less sophisticated are supposed to become less credulous as time passes, the expectation differential between the more and the less sophisticated should narrow, leading to an attenuation in the correlation between expectations and level of education.

The correlations between level of education and each expectation item are shown in Table 3. Some of these correlations are in keeping with the predictions of the expectation/disillusion theory, but the overall pattern of relationships is not at all what the theory would lead one to anticipate. By all odds the most important issue facing Carter when he assumed the presidency was whether he would be able to restore trust in government. Completely contrary to the expectation/disillusion theory, during the first several months of the Carter presidency there was a modest *positive* correlation between level of education and confidence that Carter would indeed restore confidence; that is, to the very limited degree that there was any relationship one way or the other, it was the more highly educated who held greater expectations about Carter's ability to restore trust. While this finding was not anticipated, it is easy to reconcile with the generally higher

Table 3. Correlations Between Expectations and Level of Education

	Correlation with Level of Education					
Expectation	*Apr. 1977*	*July 1977*	*Oct. 1977*	*Jan. 1978*	*Jan. 1979*	*June 1979*
Restore trust in government	.128	.095	.066	−.000	−.176	.013
Streamline the government and make it more efficient	−.100	.058	−.011	−.045	NA	NA
Balance the federal budget within the next four years	−.380	−.308	−.381	−.369	−.383	NA
Reduce unemployment to any real extent	−.018	.017	−.056	−.109	NA	NA
Keep prices from going up all the time	−.237	.202	NA	.158	NA	NA
Handle the energy problems effectively	.015	.069	−.005	−.057	NA	NA
Bring peace to the Middle East	−.178	.142	−.090	−.094	NA	NA

NOTE: The correlation coefficient is gamma. All tables from which these gammas were calculated were "equiweighted," based on the algorithm given by Bruner (1976), to correct for unequal marginal distributions on the independent variable. (Equiweighting is unnecessary for gamma in 2 × 2 tables, the basis for calculating the gammas in Table 2.) NA = not available.

levels of trust in government found among those with more education and a greater perceived stake in the political system. On most of the remaining expectation items, early expectations were simply not very closely related to level of education—the major exception involving the expectation that Carter would balance the federal budget, a naive prognosis offered disproportionately by the less educated.

Even the relationship between education and the expectation that Carter would bring the federal budget into balance did not work out wholly as predicted, however, for the anticipated attenuation in the education-based differential failed to materialize. That is, the correlation between education and the perceived likelihood that Carter would balance the budget was every bit as strong two years after Carter took office as it had been during his honeymoon period. Looking across the columns in Table 3, it is equally hard to find even the slightest hint of the predicted attenuation in the relationship between education and the remaining sets of expectations.

The irregularity of the relationship between education and expectations is further illustrated in Table 4. The CBS/*New York Times* poll employs a categorical measure of education. Therefore, the proportion of those in the lowest of the four education categories expecting Carter to perform successfully has been subtracted from the similar proportion in the highest of the four education categories in order to create a measure of the education-based "expectation gap." If presidential popularity is driven by the disillusionment of the less sophisticated, then the gap between the expectations of those in the highest and the lowest education categories should narrow over time as the less sophisticated lower their expectations. Table 4 provides little

Table 4. Expectations Gap: Difference in Expectations Between the Highest and Lowest Education Categories

	Apr. 1977	*July 1977*	*Oct. 1977*	*Jan. 1978*	*Jan. 1979*	*June 1979*
Restore trust in government	6.7%	8.0%	7.1%	.5%	−14.8%	−12.7%
Streamline the government and make it more efficient	−9.6%	−5.0%	−.7%	−3.9%	NA	NA
Balance the federal budget within the next four years	−26.9%	−18.2%	−20.9%	−18.4%	−19.3%	NA
Reduce unemployment to any real extent	−2.4%	−.6%	−4.0%	−9.6%	NA	NA
Keep prices from going up all the time	−13.8%	−10.0%	NA	−5.9%	NA	NA
Handle the energy problems effectively	5.3%	−6.8%	.3%	−6.7%	NA	NA
Bring peace to the Middle East	−13.9%	−11.4%	−5.0%	−8.0%	NA	NA

NOTE: NA = not available.

consistent support for the hypothesized relationship between education and expectations. Even in the case of Carter's ability to balance the budget, where initial expectations were most divergent, there was only a marginal narrowing of the expectation gap over time. In two more dramatic instances the gap actually widened somewhat as the initially greater expectations of the highly educated with regard to restoring trust in government and resolving energy problems declined to meet and then to fall below the expectations of the least educated.

The possibility remains, however, that the true relationship between level of education and expectations of the president is being suppressed by an uncontrolled third factor. What might this third factor be? The obvious candidate is party identification, which is known to be related to level of education (e.g., Flanigan and Zingale, 1979:75–79) and which we suspect, on the basis of Presser and Converse's work, is related to expectations as well. In the present context, the important question is not whether party identification is related to either level of education or expectations about what the president will accomplish; the question is rather whether the relationship between education and expectations will conform to the predicted pattern when party identification is held constant.

The answer, based on the findings presented in Table 5, is no. It would be pointless to discuss these correlations in any detail, for controlling for party identification does nothing whatsoever to alter the relationships shown in Table 3. That is, the correlations involving the expectation that Carter would balance the budget still start off at a moderately high level and still fail to drop off. As for the correlations involving the remaining expectation items, they continue to be either unexpectedly positive, as in the "restore trust in government" item, or to hover perilously close to zero throughout the 30-month period studied here.

Conclusion

In sum, the two main predictions of the expectation/disillusion theory have been borne out by our analysis, but the two secondary predictions derived from the theory have received very little empirical support. During the first 30 months of the Carter presidency, the steep decline in presidential popularity occurred alongside steep declines in expectations about what President Carter would be able to achieve. Moreover, those who gave positive evaluations to the president tended also to express the highest expectations of what he would accomplish. Thus, at both the macro- and micro-levels, there is evidence that expectations and popularity do indeed go hand in hand.

Table 5. Correlations Between Expectations and Level of Education, by Party Identification

	Correlation with Level of Education					
Expectation	*Apr. 1977*	*July 1977*	*Oct. 1977*	*Jan. 1978*	*Jan. 1979*	*June 1979*
Restore trust in government	.083	.223	.043	.000	−.313	−.015
	.287	.097	.118	.053	−.045	−.139
	.093	.138	.109	.020	−.129	−.131
Streamline the government and make it more efficient	.107	.079	.107	.184	NA	NA
	.037	−.020	−.099	−.028	NA	NA
	.072	−.039	−.055	−.012	NA	NA
Balance the federal budget within the next four years	−.351	−.302	−.470	−.413	−.100	NA
	−.403	−.324	−.399	−.313	−.365	NA
	.290	−.233	−.301	−.295	−.446	NA
Reduce unemployment to any real extent	.093	−.021	.062	.089	NA	NA
	−.080	−.009	−.077	.173	NA	NA
	−.019	.045	.055	−.005	NA	NA
Keep prices from going up all the time	.195	.147	NA	.316	NA	NA
	.151	.345	NA	.167	NA	NA
	.216	.107	NA	−.023	NA	NA
Handle the energy problems effectively	−.145	.067	.064	.230	NA	NA
	−.127	.037	.005	−.063	NA	NA
	−.065	.023	−.126	.079	NA	NA
Bring peace to the Middle East	.124	.088	.179	.000	NA	NA
	.019	.252	.062	.114	NA	NA
	.180	−.050	.041	.041	NA	NA

NOTE: The correlation coefficient is gamma, with tables equiweighted. The first entry for each item is the correlation for Republicans only; the second is for Democrats only; and the third is for Independents only. NA = not available.

According to Stimson, expectations started out so high as a result of a lack of sophistication, and the decline in these expectations was the immediate source of the erosion in popularity from which most presidents have suffered. However, insofar as level of education constitutes a valid measure of sophistication—a proposition about which we have some qualms even though Stimson did not raise this issue in his rejoinder to the Presser-Converse study—we have uncovered very little evidence of an education-expectation connection. Even in those relatively infrequent instances in which we can observe the expected inverse correlation, the relationship fails to behave in the anticipated fashion over time, i.e., it does not attenuate with the passage of time, as the expectation/disillusion theory predicts.

This particular configuration of findings leaves us with an unresolved issue: Why do expectations decline over time? Stimson's answer—that optimistic early expectations reflect a lack of sophistication in certain parts of the electorate, who will eventually turn against the president—has not withstood our test. We should, in fairness, concede that our data on expectations are not all that we might have wished. One could do much more, theoretically and methodologically, with the answers to a question like "What is the probability that Jimmy Carter will restore trust in government?" than one can do with a simple yes-no question. But, the yes-no questions are, at least, available and they suggest that Stimson's interpretation may still point in the right direction.

We would agree with Stimson that optimistic early expectations *do* reflect a certain lack of political sophistication. However, we see no reason to assume that this lack of political sophistication should be the province of any particular segment of the public. That is, there may be an *overriding* tendency shortly after the president has been elected to give him the benefit of the doubt—a tendency which may not be very sophisticated but which is by no means restricted to those we might label as less sophisticated.

This is fine as far as it goes, but it is at this point that we must part company with the expectation/disillusion theory. The burden of the theory is that declining presidential popularity is a function of declining expectations. But again, what causes expectations *to fall?* There is much to be said, we think, for the proposition that this decline in expectations is reality-based: the more the president fails to accomplish, the less he is expected to accomplish. But the very same logic can be invoked to explain declining presidential popularity: the more the president fails to accomplish, the lower the evaluations he receives for his performance in office. If this is true, then it would not make sense to see expectations as causally prior to evaluations, except at the very outset of a new president's term. If anything, the opposite seems the more likely possibility—that little comes to be expected of presidents who are thought to be performing inadequately.

Rather than assuming that expectations and popularity are tied together in a cause-and-effect fashion, whatever the direction of the causal flow may be, we think there is much to be said for the view that expectations and popularity are *joint outcomes* of the effectiveness with which the president is thought to be handling his job. In this context it is perhaps better to speak of a "negative coalition" made up of diverse opponents which is emboldened by disillusionment to block presidential program initiatives while being neither clever nor well-organized enough to "overthrow" him. This interpretation carries us far beyond any of the data presented in this paper, but we would be remiss if we simply accepted without question the causal framework that Stimson sketched out.

In our view, then, there is no warrant for considering declining expectations to be *the* answer to the question of why presidential popularity declines over time, even though declining expectations do appear to be related to declining popularity. We choose to interpret this relationship not as a simple causal one but rather as constituting one link in a complex chain of interacting forces. Clearly, this interactive interpretation will have to be greatly refined before it can be subjected to empirical testing. Among other things, the data employed here cannot speak to the issue of whether expectations *rise* along with approval as the next presidential election approaches, as Stimson's cyclical model suggests. What is perhaps most important at this point is the realization that a new and perhaps more complex interpretation is needed, both because of the deficiencies we have observed in the expectation/disillusion theory and because incumbent presidents and their challengers act *as if* the promise/performance nexus were important.

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Presidential Popularity and Presidential Elections: An Update and Extension

RICHARD BRODY AND LEE SIGELMAN

THIS paper updates and extends findings reported by Sigelman (1979), who discovered, contrary to earlier indications (Mueller, 1973), that the outcomes of presidential elections can be predicted with some accuracy on the basis of the president's rating in the final preelection popularity poll. The 1980 election provides an additional case to work with—the eighth time an incumbent president has sought reelection since 1938, when the Gallup presidential popularity question was first asked—and it is of obvious interest to see how closely this most recent case fits into the pattern established earlier. We also bring three previously ignored cases (the 1952, 1960, and 1968 elections) into the analysis by shifting the dependent variable from votes for the incumbent president to votes for the candidate of the incumbent president's party. This substitution is based on indications that presidential popularity has a powerful carryover effect on the outcome of midterm *congressional* elections (Tufte, 1975). If congressmen of the president's party are held responsible for the incumbent's performance, can we afford to overlook the possibility that the *presidential* candidate of the president's party is also judged accordingly?

Abstract The popularity of the president as ascertained months prior to a presidential election permits an accurate prediction of the election outcome, even when the incumbent president is not running for reelection.

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Findings

Sigelman's seven-election regression model was:

Predicted Vote
Share for $= 38.0 + .291$ Popularity ($R = .74; \bar{R}^2 = .45$) (1)
Incumbent Party $(.119)$

The expected 1952, 1960, 1968, and 1980 vote shares of the incumbent president's party, calculated from Eq. (1), are all higher than the actual vote shares, the mean deviation being 5.9 percent and the range running from 2.9 percent (1952) to 7.8 percent (1980). Despite the consistency of the overestimates, all are within striking distance; indeed, the overall difference between expected and observed votes falls well short of statistical significance (chi-square = 3.15, $df = 4$, n.s.). The model's performance is all the more impressive in light of the nontrivial third-party voting that took place in both 1968 and 1980, the two years for which the seven-election model generates the least accurate predictions. So even with these departures from presidential "politics as usual" included in the model, the four "new" elections fit the seven-election model quite well.

If we had used the first 10 elections (1940–1976) to forecast the outcome of the eleventh (1980), the prediction equation would of course be somewhat different:

Predicted Vote
Share for $= 32.2 + .368$ Popularity ($R = .81; \bar{R}^2 = .61$) (2)
Incumbent Party $(.094)$

The expected 1980 vote share for the incumbent party (the Democrats) was 4.8 percent more than Carter actually received but remarkably close to the combined Carter-Anderson vote. By comparison, Sigelman's seven-election model predicts a 48.7 percent Democratic vote share in 1980 as an outgrowth of Carter's 37 percent popularity in the final preelection Gallup Poll. Thus, bringing the three nonincumbent elections into the model actually improves the model's performance, at least insofar as the 1980 election is concerned.

In forecasting future elections, data from all 11 elections should be employed, in which case the prediction equation becomes:

Predicted Vote
Share for $= 29.8 + .406$ Popularity ($R = .84; \bar{R}^2 = .67$) (3)
Incumbent Party $(.089)$

The 11-election scatterplot is shown in Figure 1. The addition of the 1980 election and the three nonincumbent elections leads to a more

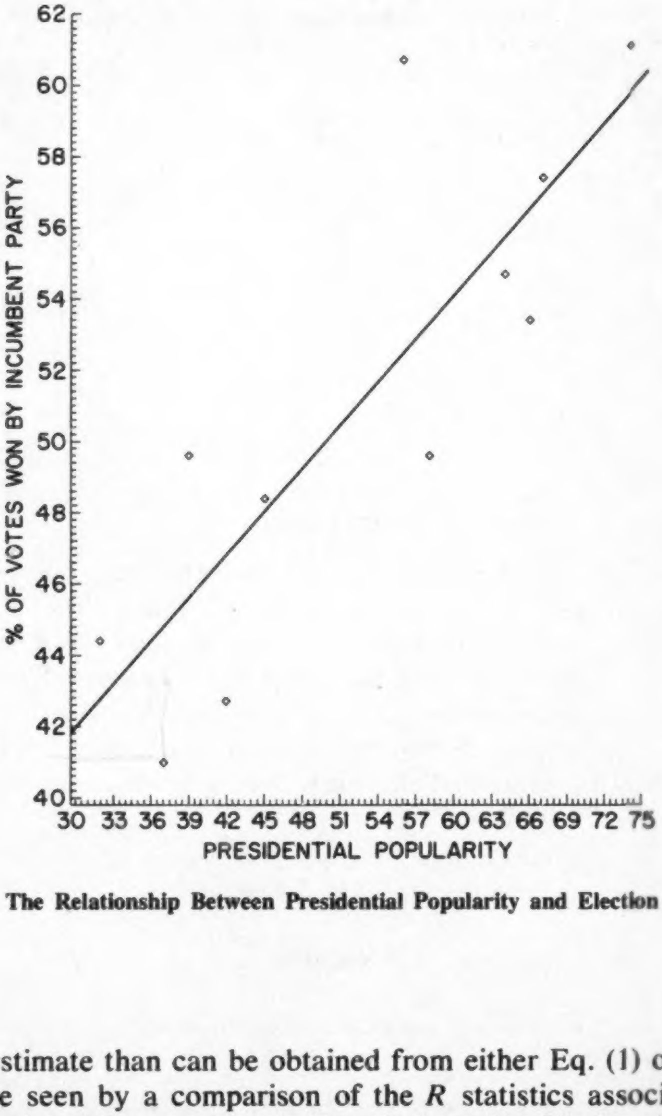

Figure 1. The Relationship Between Presidential Popularity and Election Outcomes

precise estimate than can be obtained from either Eq. (1) or Eq. (2), as can be seen by a comparison of the R statistics associated with each equation. We can also use Eq. (3) to revise Sigelman's "rule of thumb" for assessing probable election outcomes. Table 1 translates presidential popularity into the expected share of the popular votes captured by the candidate of the incumbent president's party, based on Eq. (3). Whereas Sigelman (1979:534) concluded that "an incumbent whose last preelection popularity figure is 45 percent or less can expect to lose or eke out the narrowest of victories in the popular vote column," the break-even point for a nominee of the president's party appears, on the basis of the present analysis, to be a 50 percent presidential popularity rating.

Table 1. Presidential Popularity and the Predicted Popular Vote

If Presidential Popularity in the Last Preelection Survey Stands at	*Then the Incumbent Party's Predicted Share of the Popular Vote Is*
25%	40.0%
30	42.0
35	44.0
40	46.0
45	48.1
50	50.1
55	52.1
60	54.2
65	56.2

Conclusion

The findings reported here suggest that the incumbent president *and his party* are held accountable at the time of the presidential election for the performance of the incumbent over the previous four years (Fiorina, 1981). The fact that there is a link between presidential popularity and presidential voting means that data on presidential popularity can be used to generate reasonably accurate predictions of the outcome of presidential elections, even when the incumbent is not running for reelection.

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Economic and Social Aspects of the Literary Situation

PAUL WILLIAM KINGSTON AND JONATHAN R. COLE

SURVEYS of public opinion have repeatedly found that the occupation of author is held in comparatively high esteem by the American public, ranking among the top 10 percent of all occupations (Reiss, et al., 1961). Since the prestige of an occupation is notably associated with the typical income derived from it (Blau and Duncan, 1967; Featherman and Hauser, 1978; Treiman, 1977), the prestige accorded authors might therefore suggest that Americans believe that authors receive substantial income from their writing.

But if Americans do make such an inference, it can scarcely be based on reliable information. There is little systematic data about the incomes earned by authors from their writing, nor is any more known

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Abstract On the basis of a recent large survey of American authors, we report on the general economic condition of this unstructured and differentiated occupation, as well as the economic conditions of various types of authors. With few exceptions, authors earn little from their writing; only those who can "afford" to spend long hours writing stand a reasonable chance of making a modest income from it. The unusual nature of the occupation is suggested by the fact that a wide range of the authors' personal characteristics (demographic and socioeconomic attributes), as well as seemingly important aspects of their writing careers, account for very little variation in economic success.

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about their general economic condition. The very few empirical studies of the matter have been based on sketchy data at best.[1]

On the basis of a recent large survey of American authors, we report here on the general economic condition of this disparate group, as well as the economic conditions of various types of authors. As we will discuss further, we faced the inevitably contentious issue of defining an "author" and then sought information from a representative sample of those meeting this definition. For both conceptual and practical reasons, the difficulties involved in surveying authors were considerable. However, with the cooperation of 2,241 authors who responded to the survey, we have obtained the necessary data for reasonably reliable estimates—at any rate, the best estimates yet available.

Much discussed and often publicly visible, authors have intrinsic interest. But they additionally represent an occupation which lacks the structured coherence that defines most others. If an occupation may be defined as a relatively continuous and specific pattern of economic activity providing workers with a livelihood and defining their social status (Form, 1968), this study suggests that it is difficult to portray authors as belonging to a single occupation. Many of the differences in working conditions and economic rewards seem as marked as their similarities. Indeed, the occupation of author appears as one of those "occupational groups so vague as to defy precise definition" (Caplow, 1954:63).

Nevertheless, analysis of unstructured and differentiated occupations seems essential to the emerging concern to understand patterns of success and career movement *within* occupations. As factors related to status attainment (especially as indicated by occupational attainment) have become better understood (Featherman and Hauser, 1978; Jencks, et al., 1979), some researchers have turned to fine-grained analysis of intra-occupational differentiation. To date, however, these analyses have tended to focus on comparatively structured occupations—e.g., scientists (Cole and Cole, 1973, among others), government bureaucrats (Grandjean, 1981), business executives (Kanter, 1977), and engineers (Perrucci, 1970). Analysis of this unusual occupation can complement existing research on intra-occupational attainment.

This paper has three sections. First, we briefly discuss how the population is defined and the main characteristics of the sample. We then report on the general economic conditions of authors and consider these conditions in relation to the diverse commitments authors give to writing. Finally, we analyze how various other personal characteristics and career contingencies relate to writing income.

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[1] William J. Lord, Jr.'s (1957) study is very dated and based on a small sample of dubious representativeness. PEN (1980), an organization of writers, recently conducted a small-scale study of its members, but its range of substantive concerns is limited and the response rate is low (20 percent; $N = 358$).

Defining the Population

At the outset we faced the complex and difficult question: Who is an author? Any particular definition is subject to objections. Some time ago, for example, Malcolm Cowley (1954) took a decidedly latitudinarian view in defining a writer as "a man or woman who writes." True enough, one supposes, but this does not help much to define an "author" for the purposes of a systematic survey. Does an "author" give a certain minimum commitment of time and effort to writing for publication? Is self-definition enough, or must a person have been published, or recently published, or even have earned more than a certain annual amount to qualify as an author?

Because we did not wish to foreclose the scope of the inquiry prematurely, we have defined as authors all contemporary American writers who have had at least one book published. As it turns out, all but a few of the 2,241 authors in the study had published at least one book, and these few others had published the equivalent.

The inclusive strategy had a further practical and, in the event, fundamental reason. There is no "officially recognized" directory of American authors which could serve as a basis for drawing our sample. However, the Authors Guild Foundation provided a list of members of the Authors Guild and of writers invited to join the guild who had not done so. The central requirement for guild membership is a published book or three recent magazine articles.[2] To our knowledge, this constitutes the largest and most differentiated list of American book writers now available.[3]

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[2] Guidelines for membership in the Authors Guild Inc. specify that "any author who shall have had three works, fiction or nonfiction," published by a magazine or magazines of general circulation within 18 months prior to his application is eligible to join. There are almost no writers in the sample without a published book.

[3] Of course, even this broad definition excludes some who may have a reasonable claim to being considered an author. Our definition means that every member of that population must have been successful at least once in convincing a publisher to publish his or her book. The focus on authors of *books* is also somewhat restrictive and may appear arbitrary; writers of published articles and essays who have never published a book are not included in this population. Of course, the larger portion of writers whose work is principally focused on articles also have published books, and are therefore located in the population. A sample might have been drawn from *Books in Print*, but

Whatever their definition of author, researchers must inevitably confront the problem that the characteristics of the general population are virtually unknown.[4] It is therefore clearly difficult to draw firm conclusions about the representativeness of the sample. Our claim of having a representative sample essentially rests on the assumption that the sampling frames comprise a representative though not exhaustive listing of authors as defined here.

The Sample

We sampled from two separate lists: the membership list of the Authors Guild Inc. as of fall 1979, and the guild's list of book writers who had been invited to join the guild within the interval 1976–1979 but had declined to join. Almost 5,000 authors were members of the guild in 1979; this is the largest listing of American book writers extant. The list of decliners for 1976–1979 comprised some 3,750 names. Every type of commercially published book author is qualified for membership, and the guild's invitation policy insures that almost all authors who write for a general audience are invited to join. Academic and technically oriented writers, however, usually do not receive invitations because they are not inclined to join the guild.

Considered together, these two lists make up a very large but indeterminate number of those meeting our inclusive definition. The combined lists, however, still exclude or at least undercount authors of academically and technically oriented books, and authors who had been invited to join the guild before 1976 and had not received another invitation since then. It is unclear how the underrepresentation of authors of practical or professional books affects the estimates of the "typical" author's income from writing. There is no solid evidence that this group of authors has greater financial rewards from writing than do the authors of literary books.[5]

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that did not offer us addresses. A recent publication, *The Writers Directory 1982–84*, which was not published at the time of drawing the sample, might also have been useful in developing a sampling frame.

[4] Census data are of little help. In the *Standard Occupational Classification Manual*, authors are defined as follows: "This minor group [sic] includes occupations originating and editing written material for publication in printed form and for spoken use, broadcasting, and dramatic presentation; and translating or interpreting written or spoken words from one language to another." This definition is too broad because it includes many occupations only loosely connected with writing rather than requiring actual writing, let alone publication. It is too narrow in leaving out published writers who, having other jobs as well, do not designate their principal occupation as author when filling out the census form.

[5] In deciding on sampling strategy, we could not know what proportion of the total population of authors was represented by each sampling frame and thus draw a sample so

After the foreign and deceased members of the sample, as well as all with undeliverable addresses, had been subtracted, our sample was 4,856. Our analysis is based on 2,241 completed questionnaires, a response rate of 46 percent. This response represents the return to two mailings.

This lower than desirable response obviously raises the issue of representativeness. As previously noted, however, there can be no direct test because the population parameters are unknown. Any claim to having obtained a representative sample must rest on two assumptions: that the sampling frame comprises a representative listing from the total population of authors, and that the actual respondents constitute a random sample of the original sample. It is not possible to demonstrate conclusively that the first condition has been met. But three separate checks suggest that there is no significant response bias, at least in terms of economic success:

(1) Respondents to the Authors Guild survey had a very similar mean and median income from writing to those reported in a recent small-scale survey conducted by PEN (1980). (2) We roughly computed the percentage of new *titles* that recently made one of the *New York Times* best-seller lists and compared that figure with the percentage of *authors* in our response group who had a book on one of these lists within the same period. Obviously, these percentages are not directly comparable, but it appears that we have not underrepresented the best-selling authors. (3) As an indirect test of response bias, we compared the distribution of responses in each of three mailings. (In addition to sending a follow-up survey to those not responding to the first mailing, we sent a third "mini-questionnaire" to a small systematically drawn subsample of the nonresponding members of the guild.) The value of the third mailing is limited by a low response rate—29 percent, $N = 44$. The similarity in the responses of the first and second mailings on all analyzed items is unmistakable. There are a few differences in responses to the second and third mailings; but in perhaps the most crucial item, book writing income, the difference between respondents to these mailings is not statistically significant (further details are specified in Kingston, Cole with Merton, 1981).

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that all authors would have an equal chance of inclusion. Accordingly, our concern for obtaining a sufficient response directly affected the sampling procedures. Guild members were assumed likely to be relatively cooperative and therefore were sampled at a slightly higher rate. Approximately 3,200 names were selected from the guild's membership list; the sampling ratio for members was thus about 60 percent. Somewhat fewer than 1,900 were selected from the rest of the decliners, a sampling ratio of about 50 percent. (As it turned out, the nonmembers were not distinctly different from the members, and therefore no weighting factors were used.)

Table 1. 1979 Writing-Related Income

	%	N
0– 2,499	38%	736
2,500– 4,999	13	249
5,000– 9,999	14	279
10,000–19,999	13	254
20,000–49,999	13	264
50,000–99,999	5	103
100,000+	4	75
	100	1960

General Economic Condition

For the purposes of this study, writing-related income was defined as the total earned in a calendar year from: royalties from hardcover editions, paperback originals, and paperback reprints; all other subsidiary rights; fees for freelance magazine and newspaper articles; and payments for television, radio, and movie scripts. Earnings from salaried jobs, even if the work involves writing (e.g., newspaper reporter), as well as freelance, fee-for-service editing, translating, lecturing, etc. were not included with writing-related income.

Plainly, for most authors, writing yields little economic return: the median writing-related income was $4,775. The general picture can be seen from another perspective. Table 1 sets out the proportions of authors in the sample located in seven income strata, ranging from the 10 percent who in 1979 earned nothing at all and the additional 28 percent receiving up to $2,500 that year to the 4 percent with incomes of $100,000 or more. Again, the pattern is clear: the very many on the lowest rungs receiving negligible income from their writing and the few at the top being very highly rewarded.

Furthermore, relating the number of hours that authors work at writing to their incomes from that work, we can arrive at a rough estimate of median hourly income rate in 1979: just under five dollars an hour ($4.90).[6]

For all the public notice given to the diverse and sometimes spectacularly lucrative money-making avenues open to authors, most still make virtually all of their writing-related income from books, especially from royalty payments. The typical author receives 98

[6] This is an approximate, derived figure. We estimate the hourly income here by dividing each author's total annual income directly related to writing by his/her estimated average number of hours a week spent writing, extended to a 50-week work year.

cents of every writing dollar from books, including both royalty payments and all subsidiary rights. Royalty payments alone typically account for 87 cents of every writing dollar. (The typical cases here represent the median of the ratios of authors' book-related income to total writing income and royalty payments to the latter.)[7] The median income derived from subsidiary rights was zero; only about a tenth made as much as $2,000 in subsidiary rights.

And yet analysis of the economic condition of authors is incomplete unless it considers their total personal and family incomes. Authors reported median total personal incomes of $27,000 in 1979. (This figure represents all earned and unearned income, including investments jointly held with a spouse.) The median conceals considerable variation: the lowest quarter earned less than $13,000, the top quarter more than $45,000. At the extremes of the distribution, the lowest tenth made less than $5,000 and the highest tenth, almost $80,000.

The reported total family incomes of authors—an aggregate measure of all earned and unearned income, including a spouse's for the married—further suggest the large discrepancy between the usual economic return to writing and the level of material comfort available to most authors. The median family income in 1979 was $38,000; about a tenth had incomes below $12,000, but another tenth had incomes in excess of $100,000. In short, if writing income alone would seemingly destine most writers to the garret, few would appear compelled to live so.

Quite clearly, both the large variation in economic returns to writing and the substantial difference between typical writing incomes and typical personal incomes point to the occupational differentiation within the ranks of authors.

Occupational Diversity

Authors are about equally split between those who consider themselves full-time authors or writers and those who do not. But being a full-time author may mean quite different things to different people. The authors were therefore asked to estimate the average amount of time they devoted to writing a week. (The question on this matter was deliberately worded so that authors would include research and

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[7] We caution against placing undue confidence in the exact figures relating to the ratio of royalty payments to total writing income because there are many missing cases. A number of authors reported total writing income but did not report a breakdown of their royalty payments. In all likelihood, the median ratio accurately suggests the overwhelming importance of royalty payments for most authors, but the precise degree of this importance is open to some question.

Table 2. Hours Per Week Writing, by Self-Designation as Full-time Author and by Occupational Commitment

	Self-Designation		*Occupational Commitment*	
Average Hours per Week Writing	*Full-time Freelance Author*	*Not Full-time Freelance Author*	*Hold Other Paid Job*	*No Other Paid Job*
0–9	4%	33%	28%	10%
10–19	8	34	29	13
20–39	48	29	34	43
40+	40	5	10	34
Total	100	100	100	100
(rounded)	(1038)	(1024)	(983)	(1144)

thinking time, as well as time spent on other activities that directly contributed to their writing product.)[8] At the median level, authors worked 20 hours a week at writing; the most active quarter, 35 or more hours, and the least active quarter, 10 or fewer hours. As Table 2 indicates, self-designated full- and part-time authors differ substantially in the amount of time given over to writing. Forty percent of the full-time compared to 5 percent of the part-time authors worked 40 or more hours a week.

This variation in writing hours reflects the fact that many authors hold other jobs. Almost half (46 percent) held a "position besides freelance writing" at the time of the survey (excluding occasional lectures or readings as well as irregular, fee-for-service editing, translating, and so forth). Thus, only a slight majority of authors can be occupationally defined solely by their writing activity. Quite expectedly, as Table 2 shows, the amount of time devoted to writing also substantially differs between those who hold another job and those who only write. More than three-quarters (77 percent) of the authors without another job write at least 20 hours a week; in contrast, slightly more than half (56 percent) of those holding another job work fewer than 20 hours a week, and about half of this group write fewer than 10 hours a week.

A TYPOLOGY OF COMMITMENT TO WRITING ACTIVITY

Thus, within the ranks of both full-time and part-time authors, there is considerable variation in the time committed to writing. These variable reports lead us to see several types of authors in terms of work patterns. The classification is based on two aspects of authors' work lives: whether they held a position other than writing, and the number of hours a week they spent writing.

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[8] The first question read: "Would you characterize your occupation as full-time, freelance author or writer?" The follow-up question read: "On average, in the last year how many hours per week did you spend writing or directly working in some other way on your own book or article?"

Type I is the *committed full-time* author, in the dual sense of spending at least 25 hours a week on writing and of not holding any other paid position (though in some cases earning irregular fees-for-service, writing-related income from editing, translating, and the like). Thirty-five percent of all the authors are of this kind. At the other extreme, Type V is the *marginal part-timer* (13 percent of the sample), who holds other paid positions unconnected with the writing craft and puts in fewer than 10 hours a week on writing. The three remaining types remind us that the amount of time devoted to writing and the maintenance of another paying job are not as highly correlated as we might suppose. The Type II, *limited full-timer* (19 percent of the sample), consists of authors who report spending no more than 25 hours a week at writing but hold no other regular paid job. Type III, the *committed part-timer* (another 13 percent), holds paid positions other than freelance writing, and yet averages 25 or more hours a week on writing. This group of authors testifies to the commonplace that time is not a zero-sum phenomenon. In effect, these are authors who hold two substantial positions. Finally, Type IV, the *intermittent part-timer,* represents the sizable group of authors, 21 percent of them, who hold paid positions other than freelance writing, and devote a smaller but still considerable number of hours each week to their writing.

Before turning to the incomes of these types of authors, we note some further work characteristics of the members of this disparate occupation.

FULL-TIMERS: THE ON AND OFF CAREER

By asking authors to indicate their present occupational position(s), we in effect take a collective snapshot of the occupational distribution of authors. Yet this does not provide a sense of the highly irregular career patterns experienced by many authors. As one commented on the questionnaire, "I work (that is, hold another job) only when I have to." Just a few months earlier, then, he would have been considered a full-timer; at the time of the survey he was a part-timer.

Although all such career movements cannot be traced by the data in hand, the limited information we do have on the occupational histories of the current full-timers is revealing. Most strikingly, less than a tenth of the full-timers have been full-time writers throughout their working lives. Thus, this group of career-long, full-time writers represents about 5 percent of all authors. Moreover, most of the current full-timers have relied on income from other jobs for considerable periods during their working careers; in fact, half of them have *primarily* supported themselves by other jobs for at least as many years as they have been full-time writers. Correlatively, almost half of the current part-timers have been full-timers for at least a year at one point or another.[9]

Let us be quite clear about our definition of full-time author. To reiterate: a significant number of those designated as full-timers take on other work—irregular, fee-for-service editing, translating, lecturing, etc. But this kind of work is often sporadic and often dovetails with their freelance writing.[10] We therefore decided to distinguish such patterns of work from regular, salaried employment and from professional/entrepreneurial endeavors in arriving at these types of authors. In fact, slightly more than 40 percent of both the committed and limited full-timers give at least some time to these writing-related, fee-for-service activities. For some full-timers, these activities take up a good deal of time: 16 percent of the committed full-timers and 27 percent of the limited full-timers average at least 10 hours per week in such activities.

In fact, 70 percent of all authors are engaged in some paid work other than book and article writing. Writing is an "occupation," then, in which a sizable majority is actively engaged in some other paid work. Few authors work solely at writing throughout their careers to the total exclusion of other forms of regular employment. Indeed, since almost half of all authors currently hold a regular other job, consideration of the part-timers is essential to understanding this occupation.

PART-TIME AUTHORS: THEIR OCCUPATIONS

For the part-time writers, it often makes sense to talk of their *occupations* because their work lives are spread across two or more jobs. As one harried New York-based novelist said in one of our focused interviews, her work life has meant a time-pressed scramble across the metropolitan area: from one college to another to teach classes in writing, to a publishing company for editing work, back home to take care of children and grandchildren. Many other writers have similar patterns of holding several jobs at once in order to support themselves. Although we cannot report all details of the occupational lives of this novelist and others like her, we can summarize the other kinds of paid jobs which provided part-time writers with the most income.

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[9] Of course, the direction of causality is not clear. Authors may not spend as much time on writing as they would like to because of the requirements of other work, or they may spend less time in writing because they have found it to be impossible to earn a living even putting in more time at it.

[10] The occupational histories of the full-timers also make evident that writing is a career without any clearly specified entry-level position and associated career ladder. The previous other jobs of the current full-timers are quite diverse, though the most common (27 percent) were "other professional" and "editor/publisher" (19 percent), and relatively few worked at low prestige jobs.

By far the most common ancillary occupation among part-time writers is university teaching (36 percent). The second most common kind of other work among part-time writers is that disparate category, "other professional." It includes such varied occupations as lawyers, physicians, clergy, computer programmers, and many kinds of "artistic" people. About a fifth of the part-time authors are in this occupational category. Although no great number appear in any other single category, a substantial percentage are employed in occupations which directly involve writing skills. The combined percentage of "editor/publisher," "journalist," and "public relations" is 21 percent. By and large, then, most authors who hold other jobs work in relatively prestigious occupations, with a negligible few in blue-collar or the lesser status white-collar occupations.

The mixed careers of part-time authors are usually, it seems, more a matter of economic necessity than of preference. To the question, "If you could at least match your present total income by writing full-time, would you drop your other work?" almost half (46 percent) of the part-time authors responded "yes" and another 22 percent "possibly." Furthermore, three-quarters of the committed part-time authors and about 60 percent of intermittent part-time authors report that they consider writing "much more" or "somewhat more satisfying" than their other jobs. But if these types of writers seldom expressed a preference for their other work, it is quite different among marginal part-timers. Most of these authors, 54 percent of them, considered their other work either "equally" or "more" satisfying than writing.

Plainly, writers "subsidize" their writing activity with other forms of work. Indirectly they also subsidize the publishing industry. In a recent article, James Lincoln Collier made just this claim. Using himself as an exemplifying case, Collier describes his income and expenses from the publication of his widely acclaimed book, *The Making of Jazz*, which was an American Book Award nominee, which the *New Yorker* called "an essential book," and which *Publishers Weekly* called "now the finest book of its kind."

> My income to date from the book is $27,884. There was no agent's fee, but deducting 25% for overhead and fringe, mainly retirement fund, I have $20,913 left. I also spent $1500 for photographs, $1000 for travel for research and promotion, $500 for 100 books, $1500 for 300 records, leaving $16,413. I spent two years writing the book, over a span of four years. Thus, my annual before-tax income from it was $8200. And even if you forget about overhead and fringe, I still made only $14,000 a year from the book, before taxes. In terms of time that might have been spent in more profitable ways, the book cost me several tens of thousands of dollars out of my own pocket to write (Collier, 1981).

Collier goes on to conclude, using the results of this survey:

> This is the crux of the matter: according to the Guild survey, median hourly income for writers is $4.90. Nobody else in the publishing business works for those wages. Writers know that publishing profits are down, and that editors are poorly paid. But publishers do make money, editors eat and pay their rent, printers expect to retire on a pension. Most writers have no hope of any of these things (Collier, 1981)

Occupational Commitment and Income

Understandably enough, holding another job cuts into writing time and income: the median for part-time writers was some $2,700, and for full-time writers it was almost three times as large, $8,000. To provide further detail, Table 3 indicates the distribution of each author type across seven income strata. Plainly, the committed full-

Table 3. 1979 Writing-Related Income by Author Type; Percentage Distribution by Income Category

	1979 Writing-Related Income							
Author Type	*$0– 2,499*	*$2,500– 4,99*	*$5,000– 9,999*	*$10,000– 19,999*	*$20,000– 49,999*	*$50,000– 99,999*	*$100,000+*	*Row Total*
Committed full-timers	21%	8%	16%	16%	23%	8%	7%	100% (661)
Limited full-timers	41	18	10	13	9	6	3	100% (330)
Committed part-timers	32	13	17	13	16	6	3	100% (238)
Intermittent part-timers	49	15	17	11	5	1	1	100% (403)
Marginal part-timers	64	14	9	8	3	1	—	100% (239)

NOTE: Percentages are the proportion within each author type with an income corresponding to each income category. Percentages for each type add across to 100 percent (allowing for discrepancies caused by rounding).

timers earn the most; 38 percent earned at least $20,000 from writing, a rate much higher than any other type.

As Table 4 indicates, however, average number of hours a week writing is notably more associated with 1979 writing income (and three transformations of this figure) than full-time status. The correlations with (untransformed) income, for example, are .21 and .13

Table 4. Zero-Order Correlations Between Writing Income (Raw Values and Three Transformations) and Selected Variables

	1979 Writing Income	*Log of 1979 Writing Income*[a]	*Collapsed Income*[b]	*Log of Collapsed Income*[c]	*N*
Paternal occupation[d]	−.05*	−.04*	−.04*	−.04*	1,771
Paternal education[e]	−.02*	.03*	.00*	.03*	1,903
Have relative author[f]	−.05	−.05	−.05	−.05	1,914
Education[g]	−.07	−.06	−.11	−.05	1,914
Prestige of education[h]	−.05*	−.01*	−.03*	−.01*	1,674
Race[i]	.01*	.03*	.01*	.03*	1,911
Gender[j]	.04*	.04*	.09	.04*	1,926
Hours/week writing	.21	.32	.36	.31	1,883
Hours/week other job	−.16	−.23	−.27	−.22	1,829
Full-time status[k]	.13	.18	.25	.17	1,942
Recency of publication[l]	−.13	−.39	−.21	−.39	1,646
Number of books	.13	.24	.26	.24	1,930
Honor[m]	.05	.15	.11	.15	1,757
Writing experience[n]	.00*	−.01*	.01*	−.01*	1,520
Age	−.01*	−.14	−.07	−.14	1,747
Genre fiction[o]	.13	.07	.16	.06	1,895
Adult fiction[o]	.06	−.05	.09	−.06	1,895
Adult nonfiction[o]	−.03*	.03*	−.02*	.03*	1,960
Children's books[o]	−.03*	.04*	−.04*	.04	1,960
All other genres[o]	−.10	−.06	−.13	−.06	1,895

* Not significant at .05 level.

[a] Authors reporting zero income were coded #1.

[b] All authors with incomes above $80,000 (the top 5% in the sample were coded as $80,000).

[c] Log of the values in the second transformation.

[d] 1 = blue-collar/service; 2 = lower white-collar; 3 = manager/professional.

[e] Five values: less than high school through post-college degree.

[f] 1 = yes; 0 = no.

[g] Seven values: less than high school through Ph.D.

[h] 1 = no college; 2 = nonprestigious college; 3 = prestigious college.

[i] 1 = white; 0 = all other races.

[j] 1 = male; 0 = female.

[k] 1 = hold no other job; 0 = hold other job.

[l] 1 = 1977–1980; 2 = 1970–1976; 3 = pre-1970.

[m] 1 = no award; 2 = minor award winner; 3 = major award winner.

[n] (1980—year of first book publication).

[o] Dummy variables for genre; 1 = specified genre; 0 = all other genres.

respectively.[11] While hours spent writing is at least modestly associated with writing income, the correlation is quite modest. Long hours of writing are surely no guarantee of high income. Even among the 23 percent of authors who work at least 40 hours a week at their writing, the median 1979 writing income was $16,000. Moreover, the causal nature of this relationship is far from clear. On the one hand, extra time may increase the amount of writing produced and hence prospects for financial reward. But just as conceivably, certain authors may be able to "afford" to give relatively long hours to writing because they have been financially well rewarded in the past. That is, hours writing and writing income may be the mutual concomitants of success, with additional hours providing no necessary increment in income.

Other Correlates of Writing Income

We now turn to how social backgrounds and personal characteristics, as well as attributes of authors' careers more closely connected to writing itself, are related to writing income. In Table 4, we present the zero-order correlations between several independent variables and writing income. Writing income is represented in raw values and by three transformations, all designed to reduce the effect of outliers, given the large variance and unusual skew of the distribution. Nevertheless, the size of the correlations is generally similar across all our measures of the dependent variable.

Given the great number of authors with very low income from writing, any attempt systematically to "explain" variation in income is of limited value. The overriding fact is that, whatever the authors' personal characteristics, the dollar differences separating the large majority are quite small, if not trivial. Neither parental status, nor own education, nor geographic location, nor race accounts for more than a trivial portion of the variation in income. In short, a set of social background variables that have proven to be significant correlates of achievement in the larger occupational structure are simply not associated with financial success among authors.

It is worth noting a few details about these "nonfindings." Although the correlation between education and income is slightly negative, it is probably an artifact of the limited commitment of Ph.D.'s to commercial books. Moreover, among the college-educated, the prestige of the college attended is unrelated to the economic returns from writing, a finding that contradicts the expressed view of some authors who believe that graduates of elite colleges have an advantage in dealing with publishers. The widespread belief that authors who live in the New York area, still the publishing capital of the country (Coser, et al., 1982), would do better than elsewhere, is also not sustained by our data. Where authors live and work has little to do with how much they earn; even among the small group who earned upwards of $50,000, there seems to be no disproportionate number of New Yorkers.

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[11] An OLS regression model incorporating these two independent variables, as well as others relating to authors' writing careers, points to the relative strength and absolute significance of hours spent writing. See Table 7, to be discussed more fully below.

But the now familiar findings on differences in income between men and women appear here too, although such differences are comparatively muted. For example, women authors earned a median of $4,000 from writing in 1979 compared to $5,200 earned by men. This ratio of .77 suggests a nontrivial difference and one which undoubtedly is consequential for writers themselves, but it is significantly smaller than the 40 percent difference that separates the median incomes of men and women in the labor force generally.

That the income differences between men and women authors are *relatively small* is also shown by the fact that the chances of an author being in the lowest or the highest income strata are unrelated to gender. About the same proportions of men and women (37 percent as against 40 percent) earn less than $2,500 per year. Much the same situation holds at the other extreme of the income distribution: 10 percent of the men and 7 percent of the women earn more than $50,000.[12] We also find no marked gender-related differences in income among those who work exclusively at writing, the committed full-timers who spend 25 hours a week or more at writing and have no other paid positions: 12 percent of the men and the women in this group earned $20,000 or more.

However, Table 5 reveals an interaction between hours spent writing and gender on writing income. When we consider the number of hours authors worked at writing, the ratio of female to male income varies significantly from the overall ratio of .77. For those authors who worked less than 40 hours per week at writing, the gender differences in writing-related income were less than the overall ratio. For those working less than 10 hours the female to male income ratio

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[12] Of course, we can look at these upper income figures in two ways. Either we note that there is only a 3 percent difference in the proportion of men and women authors earning over $50,000 in 1979, or we note that almost one and a half times as many men as women are in this upper income group.

Table 5. Median Writing Income, by Sex and Hours Spent Writing per Week

	0–9 Hrs.	*10–19 Hrs.*	*20–39 Hrs.*	*40+ Hrs.*
Women	$1,250 (*N* = 119)	$2,160 (*N* = 142)	$5,000 (*N* = 326)	$10,500 (*N* = 136)
Men	$1,500 (*N* = 200)	$2,325 (*N* = 230)	$6,250 (*N* = 404)	$18,600 (*N* = 297)

is .83; it is .93 for writers working from 10 to 19 hours; and .80 for those working 20 to 39 hours. However, for those men and women authors who worked at least 40 hours each week at writing, the income returns to men were considerably higher than those of women. The ratio of female to male earnings was .56. The differences between female and male earnings among the committed full-time authors are, in part, a result of the differences in the genres in which they work. Children's books tend to yield lower incomes than other forms of writing. Women are almost three times as apt as men to be primarily authors of children's books (25 percent against 9 percent).

The linear combination of these personal characteristics, including parental education, paternal occupation, close relative with a published book, race (white, nonwhite), gender, and dummy variables for religious background, accounts for only one percent of the variation in 1979 writing income.

Career Histories

Are there more compelling linkages between writing income and certain contingencies in the actual writing careers of authors? Many economists (see, for example, Becker, 1964) have stressed the importance of formal education and job experience for enhancing productivity. On the further assumption that workers are paid in accord with their marginal productivity, those relatively well endowed with "human capital" should be relatively well rewarded. Of course, as economists recognize, productivity is an elusive concept and its determinants are not always well understood. This is certainly so for writing, a largely unroutinized activity, calling for unusual, often creative skills. Its "product" is economically rewarded in light of diverse commercial-aesthetic judgments. It is not greatly surprising, then, that regression models incorporating only measures of education and job experience explain no variance in writing income. Here "job experience" is crudely measured by length of time from first book publication to the present. To increase the number of cases, we also used age as a rough indicator of "job experience"; the results are similar.

PUBLICATION HISTORIES

However, again not surprisingly, a direct measure of "productivity"—number of published books—does appear to count financially. Among authors who have published work in the three years preceding (1977–1980), the extent of publications is significantly related to income from writing in 1979 (see Table 6). To cite one example, 43 percent with two or fewer published books to their credit had incomes lower than $2,500 compared with 12 percent of prolific authors with 10 or more books published. All this is expectable, of course: the more authors continue to publish, the more apt they are to earn royalties and the more apt they are to get favorable royalty arrangements from publishers. This holds in general in spite of the conspicuous cases of certain one-book authors whose contracts, royalties, and subsidiary rights have received copious attention from the press.

Yet, apparently, even prolific authors must continue to publish if they wish to avoid substantial drops in income over the short run. For example, 58 percent of the prolific authors (10 or more books) who had *not* published in the preceding three years earned less than $5,000 from their writing in 1979 in contrast to 21 percent of equally prolific authors who were published so recently. Correlatively, among prolific authors, 39 percent of the recently published compared with 11 percent who had not published recently earned $20,000 or more from writing in 1979.

TYPES OF BOOKS

Of course, the "product" of authors is not undifferentiated in form. Authors, agents, and publishers are all apt to have strong convictions

Table 6. 1979 Writing-Related Income by Publishing Productivity by Recency of Book Publication

1979 Writing Income	*Number of Books Published*							
	*1977–1979**				*1970–1976*			
	10+	*5–9*	*3–4*	*0–2*	*10+*	*5–9*	*3–4*	*0–2*
$0–2,499	12%	23%	29%	43%	43%	53%	53%	73%
$2,500–4,999	9	17	17	16	15	13	9	8
$5,000–9,999	19	15	21	13	15	15	13	4
$10,000–19,999	21	17	10	11	17	13	9	6
$20,000–49,999	25	17	14	10	9	6	9	7
$50,000–999,999	7	7	6	4	2	1	6	2
$100,000+	7	4	4	3	—	—	1	1
Total (rounded)	100% (359)	100% (303)	100% (248)	100% (297)	100% (47)	100% (72)	100% (87)	100% (138)

* May include books published in the first quarter of 1980.

about the kinds of books which make the most money for authors. To get some sense of how these matters stand, we devised a classification of book genres which attempted to reflect types of book markets rather than one which elicited aesthetic judgments. Authors were asked to choose a single category (among 11 choices; see listing in Table 7) to which they had committed the most time in their writing careers. This represents a limitation of the data because many authors report dividing their time between and even among genres. Authors most commonly described themselves as writers of adult nonfiction—26 percent, with substantial numbers of general adult fiction—19 percent, children's books—15 percent, genre fiction (westerns, thrillers, science fiction, historical romances, and gothic/occult novels)—13 percent, and academically oriented nonfiction—12 percent.

Recency of publication, again, is notably related to writing income; accordingly, in Table 7 we restrict analysis to authors who have been published between 1977 and 1980.[13] Table 7 indicates a general pat-

Table 7. 1979 Book Writing-Related Income by Writing Genre Among Recently Published Authors (1977–80)

	$0–2,499	*$2,500–4,999*	*$5,000–9,999*	*$10,000–19,999*	*$20,000–49,999*	*$50,000–99,999*	*$100,000+*	*Total (Rounded)*
Genre fiction	17%	10	15	12	21	13	10	100% (149)
General adult fiction	29%	16	10	13	18	9	6	100% (207)
Academically oriented nonfiction	54%	14	14	10	7	0	1	100% (138)
Adult nonfiction	34%	13	15	18	13	6	2	100% (302)
How-to-books	37%	12	21	21	7	1	1	100% (68)
Technical reports/manuals/textbooks	38%	15	10	15	18	3	3	100% (40)
Poetry	59%	14	9	9	9	0	0	100% (22)
Children's books	23%	18	21	18	12	4	3	100% (196)
Religious/inspirational	25%	17	8	17	17	8	8	100% (12)
Translations	60%	0	20	0	0	20	0	100% (5)
Other	46%	13	17	17	8	0	0	100% (24)
Total (rounded)	33% (379)	14 (162)	15 (177)	15 (175)	13 (160)	6 (68)	3 (42)	100% (1,163)

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[13] Obviously, in certain categories there are too few cases for valid inference, but the distributions relating to these categories are at least suggestive. Note also, that to focus on the specific economic impact of writing genre, Table 7 classifies *book* writing-related income, not the somewhat more inclusive measure of writing-related income. As noted above, however, these figures are virtually similar for most authors.

tern of low financial returns from book authorship, but particular variations are of interest. With the single exception of genre fiction writers, about half of the authors in each category—and in some cases considerably more—earned less than $5,000 in 1979. And, in fact, the largest number within each genre received less than $2,500. Still, the genre fiction writers are *relatively* successful: almost a quarter earned in excess of $50,000. By comparison, 15 percent of the general adult fiction writers, 8 percent of the general nonfiction, and 7 percent of the children's writers reached this level. (The virtual exclusion of the "how-to" authors from the top income brackets is perhaps surprising, given the profusion of best sellers on how to run, eat, dress, cope, and so forth. However, these authors may have considered themselves part of the adult nonfiction category.)

The writers of children's books fare somewhat worse than the writers of general adult fiction and nonfiction. The children's book authors slightly exceed their adult book counterparts in the likelihood of at least earning more than $5,000, but they are less apt to reach the $20,000 mark. The contrast with adult fiction writers is noteworthy: 17 percent of the children's book authors earned more than $20,000 as compared to 33 percent of the adult fiction writers.

To see the economic return to various genres in another light, consider Table 8, which presents median 1979 writing-related income in terms of a cross-classification of genre and time commitment to writing. (Only the four most common genres are considered.) There are two important findings here. First, for each category of time commitment, there are some differences in median income figures among the genres, and this is particularly notable among writers who report working at least 40 hours a week at writing. Second, perhaps more striking, within each genre authors writing less than 40 hours a week typically earn very little from writing. Within each genre the authors who write at least 40 hours a week are typically much more successful than their counterparts who write a fewer number of hours. With a median writing-related income of $31,500, the genre fiction

Table 8. Median Writing Income, by Genre and Hours Writing per Week

	0–9 Hrs.	*10–19 Hrs.*	*20–39 Hrs.*	*40+ Hrs.*
Children's books	$2,050 (*N* = 54)	$2,964 (*N* = 69)	$7,000 (*N* = 111)	$16,100 (*N* = 41)
Adult nonfiction	$1,300 (*N* = 87)	$2,050 (*N* = 72)	$5,500 (*N* = 171)	$13,500 (*N* = 147)
Adult fiction	$ 500 (*N* = 43)	$1,400 (*N* = 60)	$5,948 (*N* = 168)	$18,000 (*N* = 79)
Genre fiction	$1,000 (*N* = 25)	$3,300 (*N* = 35)	$6,000 (*N* = 113)	$31,500 (*N* = 56)

writers who put in at least 40 hours a week are clearly the most successful group.

CRITICAL NOTICE, HONORIFIC RECOGNITION, AND INCOME

Just as members of the publishing community have strong convictions about the kinds of writing which bring authors the best money, so too are they convinced that books must be reviewed to succeed financially and that winning one or another book award is associated with greater income.

On the basis of the information at hand, we cannot say whether the frequency with which authors' books are reviewed is related to their financial return, nor can we say whether good reviews count more than bad ones. We can report that the authors in our sample whose recent books have been reviewed in the *New York Times Book Review* did better financially than those the *Times* passed by: 41 percent of authors whose books were reviewed in this one publication earned $20,000 or more in 1979 as compared with 20 percent of those not reviewed there. But being reviewed has little relation to earning a respectable income; 44 percent of the authors whose books were reviewed in the *Times* earned less than $10,000 in 1979.

Findings like these are difficult to interpret. To conclude that critical notice enhances authors' incomes is plainly unwarranted. In some measure, authors' prior literary and financial success affects their chances to be reviewed at all. And in some measure, the literary merits of some books not only bring them critical attention but also bring their authors some financial return. In the absence of the sorts of data needed to understand these complex relationships, all we can say is that being reviewed is correlated with greater income but that no causal relations are implied.

Such data on the correlation between critical notice and authors' incomes are at least consistent with prevailing wisdom. The findings we now report on authors' incomes and their having won literary awards may contravene such prevailing wisdom. In the first place, the majority of authors (54 percent) said that they had received no awards at all for their writing. Eighteen percent had received one such award, 13 percent two, and 15 percent had three or more. Furthermore, award winners earned very little more than those not honored for their work. Nor do we find that winners of several awards earn much more than those with just one.[14]

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[14] Note that this particular analysis is based on the 1979 writing incomes of recently published (1977 to early 1980) authors; thus this comparison relates awards received at various points in these writers' careers to current writing income, not the specific effects of an award on a book's sales.

But all awards are not of a piece. Some carry great prestige and publicity, others are less significant and bring little attention. Drawing upon the advice of the board of the Authors Guild, we divided the awards the authors listed into two classes, major and minor, and proceeded to examine the incomes of winners of these two classes of awards.[15]

Winners of major awards earned only a little more than winners of the minor ones: 29 percent of major and 45 percent of minor award winners earned less than $5,000; 14 percent of the major and 10 percent of the minor award winners earned more than $50,000. The zero-order correlation between log of writing income and a measure of honorific recognition (major award winner, minor award winner, no award) is .15.

Plainly, the success of authors may be related to factors such as the type and quality of the publishing house with which they are associated. Some publishing houses are more financially secure than others, some compete for authors and manuscripts, some give healthy advances and others do not. Unfortunately, we do not have data on the types of publishing houses that the authors were associated with, although there has been some recent discussion of this aspect of the publishing industry and the asymetrical relationship between authors and publishers (Coser, et al., 1982). Of course, a positive association between the "strength" of a publishing house and the economic returns to an author might only signal that better houses enlist higher quality authors.

We do know that 56 percent of the authors in our sample made use of literary agents, but that the presence of an agent does not substantially increase the probability of economic success.

SUMMARIZING CAREER FACTORS

To summarize the impact of a number of career factors on writing incomes, in Equation 1, Table 9, log of writing income is regressed

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[15] The following were designated major awards:

- Academy of American Poets
- American Academy and Institute of Arts and Letters
- Bancroft Prize
- Caldecott Medal (and Honor Books)
- International Board on Books for Young People
- Mystery Writers of America
- Edgar Allen Poe Awards (Edgars)
- National Book Awards (as of 1980 The American Book Awards TABA)
- National Book Critics Circle
- National Medal for Literature
- Newberry Medal (and Honor Books)
- George Polk Awards
- Science Fiction Writers of America (Nebula Awards)
- Pulitzer Prizes
- World Science Fiction Convention Hugo Award

Table 9. Regression Analysis of 1979 Writing Income and Log of 1979 Writing Income by Selected Writing Career Factors

	Equation 1			*Equation 2*		
	Log of Writing Income			*Writing Income*		
Independent Variables	*b*	β	SE_b	*b*	β	SE_b
Intercept	8.12	0	.29	2793.8	0	6503.11
Hours/week writing	.04	.22	.00	513.3	.14	107.12
Full-time status	.31	.05	.16	7364.0	.06	3562.55
Recency of publication	−1.75	−.33	.13	−9073.4	−.08	2890.65
Number of books	.03	.13	.00	345.0	.07	131.63
Genre fiction	.31	.03	.25	22577.5	.12	5662.89
Adult fiction	.05	.00	.21	17582.4	.11	4728.10
Adult nonfiction	.26	.04	.19	3503.1	.02	4369.70
Children's books	−.06	.00	.24	13.1	.00	5362.49
Honor	.31	.07	.11	1537.0	.02	2429.49
R^2	.25			.08		

on: (1) average number of hours a week writing, (2) full-time/part-time status, (3) recency of publication, (4) number of books, and (5) dummy variables for five genres of books—genre fiction, general adult fiction, adult nonfiction, and children's books.[16] This equation should be viewed as a rough indicator of the relative magnitude of the independent relationships of these variables and writing income, as well as a summary device to indicate their collective contribution in accounting for variance. The variables relating to occupational commitment (hours writing and full-time/part-time status) and writing productivity (recency of publication and number of books) are entered first into the equation simply because they appeared most significant.

As the standardized coefficients indicate, recency of publication, writing hours, and number of books have the strongest independent associations. (Of course, the size of the estimate for recency of publication reflects the categorization into only three values: 1977–early 1980, 1970–1976, and prior to 1970.) In conjunction with the slight associations of the other variables, they account for a quarter of the variance in the log of writing incomes.

To see the "dollar increment" associated with each of these variables, readers may also consider the unstandardized estimates in Equation 2, which regresses writing income (untransformed) on the same independent variables. Note that the R^2 is considerably reduced, only 8 percent.

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[16] Note that, because of missing observations for many variables, this regression equation is based on 1,425 cases, while there are 1,960 valid observations of writing income. Inspection of marginal distributions when categories of writing income are separately cross-classified with these factors suggests that the estimates are not appreciably affected by this reduction in number of cases.

Conclusion

The image of authors in the public mind has several apparently contradictory features. On the one hand, people perceive authors as a struggling lot, living in relative poverty, hacking away on manuscripts in cluttered garrets—but with the chance of striking it rich. On the other hand, there is the image created by the "blockbuster contract" for paperback rights, for movie rights and so forth—that of writing as a lucrative enterprise.

The data suggest that these images, like other stereotypes, are not wholly incorrect. However, the data not only substantiate the notion that most writers would live in relative poverty if they had to rely solely on their writing incomes, but also indicate that the probability of any writer's "hitting the number," or "winning the sweepstakes," is far lower than what much of the public apparently perceives. Indeed, the probabilities are so low that authors rarely continue to struggle in the state of poverty. They take other jobs that provide them with respectable incomes.

Indeed, one of our conclusions is that it is difficult to predict an author's success, from the point of view of either the author or the social scientist. It may be that individuals who elect to try their hand at professional writing tend, more than the average, to be "risk seeking professionals," whereas those who enter more traditional professional occupations tend to be more "risk adverse." All of this, of course, remains to be explored.

With few exceptions, authors earn little from their writing. Only those who can "afford" to spend long hours writing stand a reasonable chance of making a modest income from it. And even they take a considerable economic risk: long hours do not guarantee even a modest income.

That so many earn little from writing—certainly not a living wage by any estimation—directly affects the nature of the occupation. Typically out of economic necessity, and much less frequently by choice, the large majority turns to other jobs (or spouses) for economic support. Very few have made writing their single, career-long occupation. Nonetheless, most authors can amply subsidize their writing by income from other sources.

Predictably enough, this study documents the fact that recently published, prolific authors who manage to commit long hours to writing tend to do relatively well. But the unusual nature of the occupation is further suggested by the fact that a wide range of the authors' personal characteristics (demographic and socioeconomic attributes), as well as seemingly important aspects of their writing careers, account for very little variation in economic success.

If the various demographic characteristics and other attributes fail to predict the economic condition of these authors, what does? Many variables and social processes, not tapped in this study, could account, at least in part, for authors' differential financial success. The principal one, for which we have no acceptable measure that is not tautological, is talent. Beyond talent, other personal and structural factors may play an important role in determining success: Sheer physical energy and determination, self-confidence, managerial skill, and the existence of sponsors or mentors who are well connected in the publishing industry.

Then, there is, of course, the unmeasured variable of pure chance, which remains undeveloped in this study. Authors frequently speak of the role of luck or chance in their success, but it remains unclear whether this is pure luck or structured luck. Are successful authors more apt than unsuccessful ones to structure their writing lives in such a way as to increase the probability of their becoming "beneficiaries of luck"? We don't know the answers to these questions. Plainly, an adequate explanation of the financial success of authors (and, indeed, of other professionals and artists) will have to await a clearer understanding of some of the processes that lead to success, as well as improved measurement of variables that are not captured by the demographic and other personal attributes of authors.

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Response Results of Lottery Buyer Behavior Surveys: In-Home vs. Point-of-Purchase Interviews

EUGENE D. JAFFE,
HANOCH PASTERNAK, AND AVI GRIFEL

THIS paper analyzes the response results of in-home vs. point-of-purchase interviews to determine lottery purchasing behavior. National and state lotteries generate significant income in many countries, including the United States. Sales of 14 state lotteries in the United States during 1979 totaled $2.1 billion (Public Gaming Magazine, 1979). In Israel, for example, the national lottery accounts for 0.1 percent of Gross National Product and is an important revenue source for health and educational institutions. As much expertise and knowledge of consumer behavior are needed to orchestrate a successful lottery marketing program as for any other product or service, but the marketing literature has no major study on the subject. Partly for this reason, Israel's national lottery organization sponsored a series of research surveys of buyers and nonbuyers of lotteries.

For any survey, the choice of interviewing method (Rogers, 1976), interviewers, and subject matter (Johnson and DeLamater, 1976) are among the major variables which may affect response quality. Ques-

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Abstract This paper reports the response results of surveys on lottery purchasing behavior. Data are based on answers generated from two probability samples: a national, random sample of 1,646 households (in-home survey) and a point-of-purchase sample of 520 respondents. Results show that answers to the point-of-purchase survey were more accurate at half the cost of in-home interviews. The surveys were undertaken in Israel, but have implications for those wishing to sample lottery purchasers in other countries.

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tions about personal or threatening subject matter, e.g., sex behavior, use of drugs, or voting behavior, produce response effects such as overstatement or understatement of actual behavior (Blair et al., 1977; Sudman and Bradburn, 1974). Consumer surveys on the subject of lottery buying behavior may be biased because of the association of this activity with gambling. Personal interviews at the home of the respondent may produce biased responses if the interviewee has not revealed his or her purchases (or extent of purchase) to other members of the household. Therefore, recorded purchase behavior may be understated. Moreover, in area sampling of respondents, the dwelling is the sampling unit, but it is not always possible to find at home that household member who is the actual lottery purchaser. In that case, some other member may be asked to estimate the behavior of others in the household.

One solution to the problem of bias in the case of home interviews is to sample respondents at point-of-purchase locations. While this method allows the interviewer to observe and select actual buyers, it does not allow for probability sampling of nonbuyers. However, it may have the advantage of interviewing the lottery purchaser in an environment more conducive to reliable answers.

Study Objectives

To test the accurary of in-home vs. point-of-purchase surveys on the subject of lottery buying, the authors compared the results of a national survey of households with those of a survey among buyers at retail locations. Answers to several key questions are analyzed in this paper: frequency and quantity of weekly lottery game purchases, and the willingness to provide personal information. The frequency distributions of the answers to questions about lottery purchases were compared to actual lottery sales. Specifically, we investigated three questions: (1) Is there a difference in answers (to identical questions) given by respondents in home interviews vs. point-of-purchase locations? (2) If there are differences, what is their direction? (3) Which of the two sampling methods results in the most accurate answers, given cost constraints?

Study Methodology

Two surveys were undertaken to obtain information about lottery purchasing behavior. The first survey consisted of a national, random probability sample representing most of the Israeli adult population above 20 years of age. Personal interviews were held with 1,646 respondents in their homes over a four-day period in February 1980. The nonresponse rate (refusal to be interviewed) was 6 percent.

The second survey consisted of 500 interviews of lottery buyers in all major Israeli cities during October 1980. The sample size was determined on the basis of findings from previous national probability surveys conducted for the national lottery. These surveys found an average of approximately 500 lottery buyers out of samples of 1,500 households. Total interviewing time was determined on the basis of the number of people to be interviewed and the number and type of questions. It was estimated that a total of 100 hours would be needed to interview a sample of 500 respondents. The sampling procedure consisted of the following stages: selection of sampling areas, allocation of interviewing time to each area, selection of retail locations within each area, and selection of interviewing time periods.

SELECTION OF AREAS AND SAMPLING WITHIN AREAS

Seven geographical areas (see Table 1) serve as sales districts for the marketing of lottery tickets. Each district was used as a sampling area. The number of retail locations to be sampled in each area was determined in proportion to total sales volume (Sudman, 1980). Total interviewing time per location was also determined in proportion to sales volume. To illustrate, the sales volume in the Metropolitan Tel-Aviv sales district was three times the sales in the Haifa and Jerusalem sales districts. Therefore, three times the number of retail locations and interview hours were allocated in Metropolitan Tel-Aviv as in Haifa and Jerusalem. The actual retail locations in each district were randomly sampled.

TIME ALLOCATION

From a study of daily sales tallies over a period of a year, a frequency distribution of sales by day of week was determined. Generally, there was a decrease in sales the day after the lottery drawing

Table 1. Frequency of Interviews by Area, Point-of-Purchase Survey

Area Where Interview Was Held	*N Retail Outlets*	*N Interviews*	*N Interview Hours*
Metropolitan Tel-Aviv	10	257	46
Jerusalem	3	81	15
Haifa	3	82	15
Netanya	2	30	7
Beersheva	2	27	7
Petach-Tikva	1	22	4
Holon	1	21	4
	22	520	98

and an increasing sales trend until the drawing. Sales peaked in the afternoon at about 4 P.M. Morning and early afternoon sales were similar to each other. Therefore, sampling was scheduled two hours in the morning and two hours toward evening. Interviewing was undertaken on a Monday from 4 to 6 P.M. and the following Tuesday and Thursday from 10–12 A.M. These days were selected as representative of the sales data. Every retail location was sampled during these time periods. The nonresponse (refusal) rate varied from 0 to 10 percent.

Findings and Discussion

Table 2 shows the frequency distributions of lottery purchases tabulated from responses to both surveys (see Appendix for question wording). There are significant differences between the purchasing frequencies of respondents in both surveys. One explanation for the differences may be the time element, i.e., that either the number of buyers increased or that purchasing frequencies changed over the time period between both surveys. An examination of sales during this period shows an increase of 4 percent in ticket sales. This small change cannot explain the wide difference in frequencies of purchase found.

Another explanation for the differences in frequency distributions is that the probability that a lottery buyer will be sampled at a retail location increases with his or her purchase frequency. In other words, for someone who buys a lottery ticket once a week the chance of being included in the sample is four times that of someone who buys once a month. By comparison, the chances of lottery buyers being sampled in the home survey are equal regardless of frequency of purchase. It is necessary, therefore, to correct the biased data of the point-of-purchase survey in order to adjust for the difference in sam-

Table 2. Purchase Frequencies of Lottery Tickets After Correction

	Purchase Frequencies		
	Once a Week	*1–3 Times a Month*	*Infrequently (Less Than Once/Mo)*
Purchase frequency, in-home survey	29.6%	14.8%	55.6
Purchase frequency, point-of-purchase survey	58.5	21.9	19.2
Average purchase frequency of each group	1.0	0.5	0.125
Probability of being included in the point-of-purchase sample	62.0	31.0	7.0
Correction of biased purchase frequencies, point-of-purchase survey	22.8	17.1	60.1

pling probabilities. Such an adjustment can be done by weighting the sample data by inverses of the probability of selection. The corrected purchase frequencies are shown in Table 2.

In adjusting the point-of-purchase survey data, the possibility was considered that some buyers purchased lottery ticket more than once a week and therefore had multiple chances of being sampled. Data from the home survey showed that 63 percent of those buying once a week (the heavy buyers) purchased only one ticket. Most multiple purchases were made by buyers who win the instant lottery which is a feature of every weekly lottery ticket. Winning instant numbers are printed on one-third of the total emission. Rather than pocket the winnings, most of those finding a winning number use the prize (which is equal to the price of a ticket) to buy an additional ticket or tickets. Moreover, respondents in the point-of-purchase survey were asked how many times a week they purchased; 3.5 percent replied "twice a week" and none replied "more than twice." These findings led us to the conclusion that the probability of a respondent being sampled more than once was negligible. As shown in Table 2, after correction, the retail location purchase frequency data approximate[1] the national survey data.

In addition to the frequency of purchase, another key variable is the quantity of tickets purchased. Frequency distributions of ticket quantities purchased as tabulated from both surveys are shown in Table 3. There are significant differences between the two sets of distributions.[2] A downward bias is observed in the national (in-home) survey data in the reported ticket purchases of those buying two or more. For example, only 5.7 percent of total respondents identifying themselves as weekly lottery purchasers claimed to buy six or more tickets, whereas the corresponding proportion in the retail location survey is 19 percent. Similar relationships were found for most of the frequency-quantity distributions. Therefore, there is a greater propensity to understate purchases of larger ticket amounts in the in-home survey.

To determine which survey provided more accurate[3] information, extrapolations from sample data were made of the frequency and quantity of tickets bought. These extrapolations were compared to actual lottery sales during the same time period.[4] Projections of the

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[1] No significant difference at the .05 level.

[2] Significant difference at the .01 level.

[3] Accuracy is defined as the closeness to the parameter of actual sales.

[4] Extrapolations were calculated from survey data by building three-way tables consisting of the frequency and quantity of tickets purchased by age group. Projections of total purchases were derived from weighting the sample data by national population data.

Table 3. Quantity of Lottery Tickets Purchased

Purchase Frequency (i)	N Buyers	Av. Wkly Purchase Frequency f_i	Estimate of Purchase Frequency $P(E_i)$	N Tickets Purchased P_{ij}			
				6+	3–5	2	1
In-home survey							
Once/wk.	132	1.0	29.6	5.7%	8.1%	23.6%	62.6%
1–3 times per mo.	66	0.5	14.8	4.4	8.5	9.5	77.6
Infrequently (less than once/mo.)	247	0.125	55.6	0.8	1.7	11.2	86.4
Point-of-purchase survey							
Once/wk.	97	1.0	22.8	19.0	22.0	26.0	33.0
1–3 times per mo.	73	0.5	17.1	1.0	12.0	25.0	61.0
Infrequently (less than once/mo.)	282	0.125	60.1	3.0	11.0	18.0	68.0
Average N tickets purchased by group (Aj)				6	4	2	1

NOTE: The average number of tickets purchased per week for both surveys was calculated according to the formula:

$$\sum_{i=1}^{3} \sum_{j=1}^{4} P_{ij}A_jP(E_i)f_i$$

This formula was applied separately to the two data sets: in-home survey and point-of-purchase survey.

in-home survey data indicated a total of 388,000 ticket sales per week, compared to an actual average of 468,000 tickets sold per week during the month in which the interviewing took place. The retail (point-of-purchase) survey data projected to 495,000 ticket sales per week, compared to 488,000 actual average weekly sales during the same time period. Compared to actual sales, projected sales based on in-home survey data were understated by 20 percent. On the other hand, projected sales of point-of-purchase survey data were overstated compared to actual sales, but by only 1 percent.

Accuracy is only one variable to consider in choice of survey method. Another variable is cost. Our experience in Israel showed that, with an 80-question interview, a national probability survey costs $4 per interview, whereas a point-of-purchase survey costs $2 per interview, or half as much.

Response Effects

There was no difference in the willingness of respondents to both surveys to answer questions about income, age, and occupation. Eighty-nine percent of all respondents in the in-home survey answered the questions about income, compared to 88.5 percent in the retail (point-of-purchase) survey. Nearly all respondents (99 percent) of both surveys answered the question about age, and 95 percent of the respondents to the in-home survey and 97 percent of the retail survey respondents answered the question about occupation.

We were not able to check the reliability of the answers to the above questions because the retail survey included buyers only. Therefore, no comparisons could be made to national income, age, or occupation distributions.

Conclusions

We have shown that accurate data on the purchasing habits of lottery buyers can be obtained from personal interviews at point-of-purchase locations and from in-home interviews. Information on purchase frequency and quantity of tickets bought was more accurate from point-of-purchase interviews and less costly to obtain. Therefore, if information is desired from buyers only, interviewing at point-of-purchase locations is recommended.

Beyond the specific example given in this paper, point-of-purchase surveys have more general application in both marketing and public opinion research, particularly where personal and threatening subject matter may bias response. And even when nonthreatening questions are asked, it may be preferable to do the survey at the place where behavior occurs—e.g., in shopping centers, concert halls, parks, and museums—so that memory error is reduced.

Point-of-purchase interviewing could also be used to survey the characteristics and attitudes of persons who attend places that, although not illegal, are socially questionable or undesirable, such as taverns, racetracks, x-rated movie theaters, and gambling casinos. For example, few in-home survey results about liquor consumption approximate actual sales figures. The difference in the figures may be attributable not only to respondent bias but also to the fact that appreciable liquor consumption is accounted for by individuals not represented in households, such as military personnel, migrants, and rooming-house residents. Of course, surveying at the locations mentioned above should be subjected to the same rigor described in this paper.

Appendix

IN-HOME LOTTERY SURVEY

Generally, do you buy Weekly lottery tickets? (yes, every week; several times a month, 2–3 times a month; once a month; infrequently; no, never)

How many Weekly tickets did you buy last week? (more than 10 tickets; 7–9 tickets; 6; 5; 4; 3; 2; one ticket; do not buy at all; don't remember, don't know)

Generally, when you buy a weekly ticket, how many tickets do you buy for each draw? (more than 10; 5–10; 3–5; 2 tickets; one ticket; I don't buy weekly tickets)

How much did you spend on Lotto forms last week? (more than IL281; IL141–280; IL85–140; IL56–84; IL55; IL28; I did not fill out a Lotto form last week; I do not buy Lotto tickets at all)

How much did your family (not including yourself) spend on Lotto forms last week? (more than IL281; IL141–280; IL85–140; IL56–84; IL55; IL28; family members did not fill out a Lotto form last week; family members do not buy Lotto tickets at all)

Do you or some family members buy Instant lottery tickets? (yes, every week; several times a month; once a month; infrequently; no, never)

How many Instant tickets did you buy last week? (more than 10; 5–10; 3–4; 2; one ticket; I do not buy Instant tickets)

How many Instant tickets did your family buy (not including yourself) last week? (more than 10; 5–10; 3–4; 2; one ticket; none)

Do you fill out Sportoto forms? (yes, every week; several times a month; once a month; infrequently; no, never)

Does someone in your family fill out Sportoto forms? (yes, every week; several times a month; once a month; infrequently; no, never)

How much did you and your family spend on Sportoto tickets last week? (more than IL500; IL300–500; IL200–299; IL100–199; IL60–99; IL30; we didn't buy Sportoto tickets last week; don't know)

POINT-OF-PURCHASE LOTTERY SURVEY

With what frequency do you buy Weekly lottery tickets? (every week; 2–3 times a month; once a month; infrequently; never)

(To those who answered "every week") How many times a week do you buy Weekly lottery tickets? (once a week; twice a week; more than twice a week)

How many tickets do you generally buy? (more than 10; 6–9; 4–5; 3; 2; one; do not buy at all)

With what frequency do you buy Lotto? (every week; 2–3 times a week; once a month; infrequently; never)

How much do you generally spend on Lotto tickets? (more than IL480; IL241–480; IL240–145; IL144–97; IL96; IL48; I don't fill out Lotto tickets)

With what frequency do you buy Instant lottery tickets? (several times a week; every week; 2–3 times a month; once a month; infrequently; never)

How many Instant lottery tickets do you usually buy? (5 or more; 3–4; 2; one; I don't buy Instant tickets)

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The Polls:
Attitudes Toward Unemployment

CONNIE DE BOER

THE WESTERN economies have had to cope with difficulties since the early 1970s. Economic problems—specifically inflation, cost of living, high interest rates, unemployment—have been mentioned most frequently for years, in the United States as well as in Great Britain, in response to the question regularly asked in opinion polls: "What is the most important problem facing the country today?" In 1973, it was crime and in 1974 the energy crisis that were mentioned with the greatest frequency in the United States as the most important problem (AIPO). But by 1982 unemployment had superseded inflation as the "most important problem" in the United States (AIPO), and it has been the most important problem in all opinion polls conducted in Great Britain since July 1980 (MORI, SOC).

In the course of 1982, 49 percent of employed Americans considered it not at all likely that they would become unemployed within a year, whereas 66 percent had thought so in 1979 (AIPO). In January 1983, only 27 percent of the population were still convinced that it was not at all likely that an adult in the family would be out of work within a year (CBS).

The chances of quickly obtaining a job in the eventuality of unemployment are likewise rated progressively poorer. In 1975, 62 percent answered yes to the question whether one would be able to find another job fairly quickly in case of unemployment, but in 1982, only 20 percent thought so (AIPO). This decline in employment confidence had possibly already occurred before 1975 in Great Britain, where the expectations of employed people of quickly finding another job in the case of becoming unemployed declined from 38 percent in 1975 to 32 percent in July–August 1982 (SOC).

According to a CBS survey of respondents in the United States who had had personal experience with unemployment in the year preceding the investigation (Jan. 1983), the consequences of unemployment most frequently mentioned were: "cutting back on the quality of the food bought" and "taking money out of savings in order to make ends meet" (63 and 53 percent, respectively); 47 percent stated that the family had more conflicts and arguments than usual during this period. Similarly, in Great Britain, "difficulty in making ends meet on unemployment benefit" (43 percent) and "running into debt" (40 percent) were the two most serious unpleasant things associated with being unemployed

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(MORI, 1976 and 1977). These results are now some years old, but according to the response to a question regularly asked in Great Britain, 33 percent of the population in 1982 felt the unemployment benefits were too low, whereas 12 percent had thought so in 1977 (SOC, 1975–32).

The people in Great Britain have been regularly asked since 1972 what they felt to be the cause of the high unemployment figures, but the only clear trend in the responses is that fewer and fewer people put the blame on the employers (SOC, 1972–80; NOP, 1982). The government and the world recession were seen as the main culprits in 1982 (NOP).

As early as 1980, 55 percent of Americans were in favor of a large-scale government jobs program. In 1983, no less than 74 percent considered it necessary for the federal government to create jobs, even if it meant increasing the federal budget deficit (CBS). Nevertheless, only 32 percent were convinced that the government was able to reduce unemployment, whereas 63 percent felt that there was no choice but to wait for the economy to work itself out (CBS). In the Netherlands, too, a majority expected the government plans to have little effect on reducing unemployment (NIPO, 1975–82).

We are indebted to the following research organizations for their permission to publish results of surveys conducted by them:

AIPO—American Institute of Public Opinion, USA
CBS—CBS News/New York Times Poll, USA
Demoskopie—Institut für Demoskopie Allensbach, West Germany
Harris—Louis Harris and Associates, USA
MORI—Market and Opinion Research International, Great Britain
NIPO—Netherlands Institute of Public Opinion, Netherlands
NOP—NOP Market Research Ltd., Great Britain

The dates given with the tables refer to the dates the interviews for the research were held. All results are based on nationwide representative samples of adults, unless otherwise specified in the tables. In those cases where the percentages do not add up to 100 percent and there is no sign (*m*) indicating the question allowed for multiple answers, the discrepancy is due to rounding. For additional information write to: The Polls Archives, Weteringschans 100–102, 1017 XS Amsterdam, Netherlands.

Unemployment

USA—CBS: Which do you think is a more important problem facing the country today—unemployment or inflation?

	Unemployment	*Inflation*	*Both (vol.)*	*No Opinion*	
Apr. 1976	55%	33%	—	12%	
Apr. 1977	34	50	13	3	
Apr. 1980	19	73	7	1	
June 18–22, 1980	31	50	16	3	*n* = 1517
Aug. 2–7, 1980	27	54	18	1	*n* = 1769
Oct. 16–20, 1980	29	56	13	2	*n* = 1539
Nov. 7–12, 1980	24	59	14	3	*n* = 2651
Jan. 11–15, 1982	47	39	13	1	*n* = 1540
May 19–23, 1982	53	29	17	1	*n* = 1470

USA—AIPO: Do you think that your present job is safe or do you think there is any chance you may become unemployed?

Based on employed persons	*Safe*	*Not Safe*	*No Opinion*	
Oct. 3–6, 1975	74%	18%	8%	$n = 1558$

USA—CBS: How worried are you that you or someone in your household might lose a job in the next six months—very worried, somewhat worried, or not worried?

	Very	*Somewhat*	*Not Worried*	*No Opinion*
June 18–22, 1980	15%	20%	64%	1%

USA—AIPO: Thinking about the next 12 months, how likely do you think it is that you will lose your job or be laid off—very likely, fairly likely, not too likely, or not likely at all?

Employed only	*Very Likely*	*Fairly Likely*	*Not Too Likely*	*Not at All Likely*	*No Opinion*
Apr. 4–7, 1975	4	6	24	61	5
Nov. 1979	3	9	18	66	4
May 16–19, 1980	6	8	24	60	2
Jan. 1982*	5.5	11	25.5	56	2
June 11–14, 1982*	7	7	28	54	4
Nov. 5–8, 1982*	9.5	9.5	28	49	4

* Question submitted to representative sample of overall population; percentages adjusted by author for "employed only."

USA—CBS: How likely do you think it is that sometime within the next 12 months an adult in your family will be out of work and actively looking for a job—very likely, somewhat likely, not too likely, or not at all?

Jan. 16–19, 1983 $n = 1597$

Very likely	27
Somewhat likely	17
Not too likely	20
Not likely at all	27
Someone out of work (vol.)	3
Not applicable (vol.)	3
No opinion	3

USA—AIPO: In case of becoming unemployed, do you think you would be able to find a new job fairly quickly, or not?

Based on employed persons	*Yes*	*No*	*No Opinion*	
Oct. 3–6, 1975	62%	31%	7%	$n = 1558$

If you were to lose the job you now have, do you think it would be easy to get another job as good, or would it be difficult?

Based on employed persons	*Easy*	*Difficult*	*Not Sure*	
Apr. 2–5, 1982	20%	77%	3%	$n = 1058$

Would you be prepared to accept less money or lower status to get another job?

	Yes	*No*	*Depends (vol.)*	*Not Sure*	
Apr. 2–5, 1982	67%	23%	8%	2%	$n = 1058$

Suppose you were faced with the choice of losing the job you now have or taking a pay cut of 10 percent—which would you do?

	Take 10% Cut	*Lose Job*	*Not Sure*	
Apr. 2–5, 1982	85%	11%	4%	$n = 1058$

USA—CBS: If the chief wage earner in your family were to lose his or her job within the next few months, how likely do you think it is that he or she would be able to find an equally good job fairly quickly—very likely, somewhat likely, not too likely, or not likely at all?

	Very Likely	*Somewhat Likely*	*Not Too Likely*	*Not Likely at All*	*No Opinion*	
Jan. 16–19, 1983	17%	17%	27%	33%	6%	$n = 1597$

Great Britain—SOC: Do you think that your present job is safe or do you think that there is any chance you may become unemployed?

Based on employed persons	*Safe*	*Chance of Becoming Unemployed*	*Don't Know*
Oct. 1–6, 1975	62%	29%	9%
Feb. 4–9, 1976	64	26	10
Mar. 3–8, 1976	68	25	7
Aug. 4–9, 1976	71	24	5
Sep. 15–20, 1976	67	25	8
Sep. 29–Oct. 4, 1976	70	24	6
Sep. 14–19, 1977	71	20	9
May/June, 1980	68	27	5
July 1980	64	30	7
Sep. 10–15, 1980	58	38	4
Nov. 1980	66	27	7
Jan. 6–12, 1982	66	29	5
July 27–Aug. 3, 1982	67	28	5

In the case of your becoming unemployed, do you think you would be able to find a new job fairly quickly or do you think it might take longer?

Based on employed persons	*Fairly Quickly*	*Longer*	*Don't Know*
Oct. 1–6, 1975	38%	44%	18%
Jan. 28–Feb. 2, 1976	42	43	15
Mar. 3–8, 1976	42	43	15
Aug. 4–9, 1976	42	44	14
Sep. 15–20, 1976	45	41	13
Sep. 29–Oct. 4, 1976	42	45	13
May/June 1980	48	44	7
July 1980	45	47	8
Sep. 10–15, 1980	38	54	8
Nov. 1980	32	56	12
Jan. 6–12, 1982	31	61	9
July 27–Aug. 3, 1982	32	61	8

Would you be prepared to accept less money or lower status to get another job?

Based on employed persons	*Yes*	*No*	*Depends*	*Don't Know*
Jan. 6–12, 1982	64%	20%	11%	4%
July 27–Aug. 3, 1982	64	23	11	2

Great Britain—MORI, for Granada TV: If the only way you could get a job was to move away from this town, how willing would you be to move?

Based on unemployed youth, 16–21	*Very Willing*	*Fairly Willing*	*Not Very Willing*	*Not at All Willing*	*Don't Know*	
Aug. 17–24, 1981	33%	30%	16%	18%	3%	$n = 580$

Netherlands—NIPO: Do you believe that you (your husband) may possibly be out of work within a year, will definitely be out of work, or will continue being employed?

	Definitely Out of Work	*Possibly Out of Work*	*Keep Job*	*No Opinion*	
June 1976	2%	6.5%	88%	3.5%	$n = 708$
Sep. 1976	2	6.5	90	1.5	$n = 772$
Nov. 29–30, 1976	1	5	91	3	$n = 955$
Nov. 7–8, 1977	1	4	84	11	$n = 890$

What do you expect for yourself: much chance of being unemployed this year, little chance, or are you certain of keeping your job?

	Much Chance of Unemployment	*Little Chance*	*Certain of Keeping Job*	*Don't Know*	
Jan. 12–13, 1981	6%	17%	46%	31%	$n = 1111$

West Germany—Demoskopie: What changes do you feel unemployed people should be willing to accept to obtain a job?

May 24–June 16, 1982 $n = 1107$

Earning a little less	70%
Less favorable working hours	64
Work below level of training received	56
Travel considerably longer distance to place of work	55
Doing work carrying a lower status than previous job	54
Accept a temporary job	52
Accept work for which they have not been trained	44
Accept less favorable working conditions, such as noise and dirt	24
Relocating	22
None of these	7
	(m)

The Impact of Unemployment

USA—CBS: In the last 12 months has any adult in your household been out of work and actively looking for a job?

	Yes	*No*	*No Opinion*	
June 1980	28%	71%	1%	
Jan. 1982	32	67	1	
Jan. 16–19, 1983	36	64	1	$n = 1597$

What happened to your family while this person was unemployed? During that period, did your family—

Jan. 16–19, 1983	*Yes*	*No*	*No Opinion*
Take money out of savings in order to make ends meet?	53	44	3

Miss payments on your home, apartment, car or some other loan?	32	66	1
Borrow money?	33	67	—
Cut back on the quality of the food you buy?	63	37	—
Cut back on medical and dental treatments?	50	50	1
Receive food stamps or welfare?	18	82	—
Consider moving to another part of the country where there might be jobs?	29	71	—
Have more conflicts and arguments than usual?	47	50	3
Ever receive unemployment checks?	48	52	—

Great Britain—SOC: Do you think the unemployment benefit is too high, too low, or about right?

	Too High	*Too Low*	*About Right*	*Don't Know*
Oct. 1–6, 1975	34%	17%	29%	21%
Feb. 4–9, 1976	35	14	29	22
Mar. 3–8, 1976	39	12	31	18
Aug. 4–9, 1976	37	9	33	20
Sep. 15–20, 1976	39	10	33	18
Sep. 29–Oct. 4, 1976	37	12	29	22
Sep. 14–19, 1977	41	12	30	17
Jan. 6–12, 1982	5	33	19	42

Great Britain—MORI: Here is a list of some unpleasant things often associated with being unemployed. Which two or three would you say are the most serious these days? Which do you think are not as serious as they used to be?

	Sep. 7–12, 1976 (For Granada TV) n = 2011		*Oct. 25–26, 1976 (For The Daily Express) n = 1070*	
	Most Serious	*Not as Serious*	*Most Serious*	*Not as Serious*
Difficulty in making ends meet on unemployment benefit	40	23	43	19
Running into debt	41	11	40	9
The fact that the longer you are unemployed the more difficult it is to get a job	36	4	40	6
Losing the will to work	40	5	35	6
Boredom	35	9	27	11
Feeling a burden on the family	23	10	21	9
Hostility from people who think the unemployed are lazy	15	13	18	11
Signing on at unemployment exchange	11	38	6	45
Embarrassment of meeting friends and neighbours	5	44	6	40
	(*m*)	(*m*)	(*m*)	(*m*)

Causes of Unemployment

USA—CBS: What do you think is the main cause of unemployment—not enough jobs to go around, people not having the right kind of skills, or people just not wanting to work?

Jan. 11–15, 1982	*n* = 1540	
	Not enough jobs	51%
	Not right skills	21
	Not wanting to work	26
	Foreign influence	1
	Government	1
	Other/no opinion	10
		(*m*)

Who or what is to blame for unemployment in this country? Would you place a lot of the blame or not much blame for unemployment on . . .

Jan. 16–19, 1983 *n* = 1597	*A Lot*	*Not Much*	*No Opinion*
The high cost of labor in this country?	61%	31%	8%
Ronald Reagan's policies?	41	52	7
People not wanting to work?	40	56	4
Competition from foreign products?	64	31	5
What was done to reduce inflation?	41	44	15
Previous Democratic policies?	42	47	11
High interest rates?	70	26	4
People not having the right kind of skills?	40	57	3
Poor business management?	48	47	5

Great Britain—SOC: Which, if any, of these things is the main cause of the present high unemployment figures?

	Trade Unions	*World Economic Pressures*	*People Not Wanting to Work*	*Government*	*Employers*	*Other/ Don't Know*
Jan. 1972	24%	14%	14%	23%	19%	13%
Apr. 30–May 5, 1975	26	19	16	12	14	18
Oct. 1–6, 1975	34	33	25	16	10	12
Feb. 1976	37	25	22	15	4	15
Mar. 3–8, 1976	29	29	21	15	5	12
Aug. 4–9, 1976	21	32	28	21	7	14
Sep. 14–19, 1977	38	31	33	19	9	12
May/June 1980	25	35	23	23	5	9
July 10–14, 1980	28	34	22	24	6	11

Great Britain—NOP, conducted for the Daily Mail: Unemployment is currently running at its highest level since the 1930s. What do you think is mainly to blame for this?

Sep. 29–30, 1980	*n* = 1057	
	Government	32%
	High wage demands	16
	Unions	16
	World recession	14
	Low productivity	9
	High interest rates	7
	Management	5
	Public spending cuts	4
	Other reasons	19
	Don't know	6
		(*m*)

Great Britain—NOP, for The Observer: Over 3 million people are currently registered as unemployed. What do you think is mainly to blame for this?

			Unemployed in Last Year	
Aug. 31–Sep. 1, 1982	$n = 1056$	*All*	*Yes*	*No*
The government		31%	42%	29%
World recession		20	13	22
High wage demands		6	4	7
Laziness		5	2	7
Unions		4	6	4
Low productivity		3	2	3
Don't know/other		31		

West Germany—Demoskopie: Among the people who are now unemployed, do you believe there are many who just do not want to work, or do they form isolated cases?

	Oct. 1975	*Jan. 1981*	*Mar. 1982*	*May 24–June 16, 1982*
Many	49%	58%	48%	40%
Isolated cases	45	32	45	51
Undecided, no opinion	6	10	7	9
				$n = 1107$

Solutions

USA—CBS: Do you favor or oppose a large-scale government jobs program to combat unemployment?

	Favor	*Oppose*	*No Opinion*	
Aug. 2–7, 1980	55%	35%	10%	$n = 1769$

Even if it means increasing the size of the federal budget deficit do you think it is necessary at this time for the federal government to create jobs for the people who are out of work?

	Yes	*No*	*No Opinion*	
Jan. 16–19, 1983	74%	23%	3%	$n = 1597$

If it would reduce unemployment significantly, would you be willing to give back to your employer two weeks salary a year?

	Yes	*No*	*No Opinion*	
Jan. 16–19, 1983	58%	33%	9%	$n = 1597$

Do you think the government can reduce unemployment to any real extent, or do we just have to wait until the economy works itself out?

	Government Can Reduce	*Work Itself Out*	*No Opinion*	
Jan. 16–19, 1983	32%	63%	5%	$n = 1597$

Do you think it's necessary that unemployment go up, at least for a while, in order to bring inflation under control?

	Yes	*No*	*No Opinion*	
Oct. 23–28, 1982	41%	47%	12%	$n = 1437$

Great Britain—SOC, for BBC: Would you, yourself, be prepared to pay more taxes to help the unemployment or not?

	Yes	*No*	*Depends*	*Don't Know*
Jan. 6–12, 1982	30%	50%	14%	7%

Great Britain—SOC: Which do you think the government should give greater attention to—trying to curb inflation or trying to reduce unemployment?

	Inflation	*Unemployment*	*Don't Know*
Apr. 23–28, 1975	65%	26%	9%
Sep. 3–8, 1975	61	33	7
Feb. 4–9, 1976	53	40	8
Mar. 3–8, 1976	59	35	6
Aug. 4–9, 1976	51	41	8
Sep. 15–20, 1976	54	38	8
July 1980	38	54	8
Sep. 10–15, 1980	33	59	8
Nov. 1980	30	62	8
Jan. 6–12, 1982	23	70	8
July 27–Aug. 3, 1982	22	68	9
Nov. 1982	21	73	6

Great Britain—SOC, for BBC: Some people also say that those in work should be prepared to accept changes, for example, no overtime or work a 4-day week, in order to share work. Do you agree or disagree with this?

	Agree	*Disagree*	*Don't Know*
Jan. 6–12, 1982	76%	16%	8%

Great Britain—MORI, for Granada TV: Here are a number of suggestions that have been made of ways in which the level of unemployment among young people could be reduced. Which of these would you favor?

Based on unemployed youth, 16–21 $n = 580$	*Aug. 17–24, 1981*
Lower the retirement age to create more vacancies for young people	50%
Stop moonlighting (people having more than one job)	25
Guarantee unemployed school-leavers a place in a job training scheme	24
Shorten the working week	10
Restrict overtime work	10
Introduce work sharing (two people sharing one job)	9
None of these	4
	(*m*)

Great Britain—NOP, for The Observer: What steps do you think would help cut unemployment?

Aug. 31–Sep. 1, 1982 $n = 1056$

Based on all who think steps can be taken to reduce unemployment (= 526)

Lower the retirement age	19%
Increase public spending	13
More job creation schemes	12
More retraining-reeducation	9
Encourage job sharing	9
Limit pay raises	8
Import controls	8
Cut interest rates	7
Cut taxes	6
Cut inflation	5
Reduce public spending	2
Other	31
Don't know	11
	(*m*)

Netherlands—NIPO: What relief do you expect from the government schemes to combat unemployment in our country? Will unemployment be increased or reduced by them, or will they not make any difference?

	Increased	*Reduced*	*No Difference*	*No Opinion*	
Sep. 22–23, 1975	58%	9%	24%	9%	*n* = 978
1976	43	16	35	6	
Sep. 26–27, 1977	61	8	26	5	*n* = 1059
Sep. 25–29, 1978	50	11	30	9	*n* = 871

Do you believe the government measures are likely to have much, little, or no effect on unemployment?

	Much Effect	*Little Effect*	*No Effect*	*No Opinion*	
Apr. 28–29, 1981	29%	52%	13%	6%	*n* = 914

Do you feel that (——) will have much, little, or no effect on reducing unemployment?

June 14–15, 1982 *n* = 1075	*Much Effect*	*Little Effect*	*No Effect*	*No Opinion*
Government measures	16%	59%	21%	4%
Introducing more part-time jobs	31	44	21	4
A general reduction of incomes	19	47	29	5
A relief of burdens for business and industry	38	39	16	7
The activity of trade unions	16	52	27	5

West Germany—Demoskopie: Do you believe we will be able during the next three or four years to reduce substantially the current number of almost two million unemployed, or is such a result unattainable in the foreseeable future?

May 24–June 16, 1982 *n* = 1107	
Unemployment can be reduced	34%
Cannot be reduced	45
Undecided	21

There are jobs in which work, responsibility and salary are shared by two persons who decide between themselves when they will be working, that is, alternating mornings and afternoons, or relieving each other every other day or week. They would naturally also be able to arrange for taking each other's place within their joint working hours. Do you like this idea, or do you not consider it a good plan?

Based on employed persons

	Like the Idea	*Not a Good Plan*	*Depends*	*Undecided*	
Jan. 1981	38%	38%	15%	9%	
Feb. 5–15, 1983	55	26	12	7	*n* = ±1000

And if this possibility were offered you at your job, would you like to work according to such a scheme, or would you not be interested?

	Would Like	*Not Interested*	*Undecided*	*Impossible in My Work*	
Feb. 5–15, 1983	35%	39%	10%	16%	*n* = ±1000